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PROBLEMS OF ANIMAL MORPHOLOGY[1]

IN choosing a subject for the address with which it is my duty, as president of this section, to trouble you, I have found myself in no small embarrassment. As one whose business it is to lecture and give instruction in the details of comparative anatomy, and whose published work, *qualecunque sit*, has been indited on typical and, as men would now say, old-fashioned morphological lines, I seem to stand self-condemned as a morphologist. For morphology, if I read the signs of the times aright, is no longer in favor in this country, and among a section of the zoological world has almost fallen into disgrace. At all events, I have been very frankly assured that this is the case by a large proportion of the young gentlemen whom it has been my fate to examine during the past two years; and, as this seems to be the opinion of the rising generation of English zoologists, and as there are evident signs that their opinion is backed by an influential section of their elders, I have thought that it might be of some interest, and perhaps of some use, if I took this opportunity of offering an apology for animal morphology.

It is a sound rule to begin with a definition of terms, so I will first try to give a short answer to the question "What is morphology?" and, when I have given a somewhat dogmatic answer, I will try to deal in the course of this address with two further questions: What has morphology done for zoological science in the past?

[1] Address to the Zoological Section of the British Association for the Advancement of Science, Sheffield, 1910.

What remains for morphology to do in the future?

To begin with, then, what do we include under the term morphology? I must, first of all, protest against the frequent assumption that we are bound by the definitions of C. F. Wolff or Goethe, or even of Haeckel, and that we may not enlarge the limits of morphological study beyond those laid down by the fathers of this branch of our science. We are not—at all events we should not be—bound by authority, and we owe no allegiance other than what reason commends to causes and principles enunciated by our predecessors, however eminent they may have been.

The term morphology, stripped of all the theoretical conceptions that have clustered around it, means nothing more than the study of form, and it is applicable to all branches of zoology in which the relationships of animals are determined by reference to their form and structure. Morphology, therefore, extends its sway not only over the comparative anatomy of adult and recent animals, but also over paleontology, comparative embryology, systematic zoology and cytology, for all these branches of our science are occupied with the study of form. And in treating of form they have all, since the acceptance of the doctrine of descent with modification, made use of the same guiding principle—namely, that likeness of form is the index to blood-relationship. It was the introduction of this principle that revolutionized the methods of morphology fifty years ago, and stimulated that vast output of morphological work which some persons, erroneously as I think, regard as a departure from the line of progress indicated by Darwin.

We may now ask, what has morphology done for the advancement of zoological science since the publication of the "Origin of Species"? We need not stop to inquire what facts it has accumulated: it is sufficiently obvious that it has added enormously to our stock of concrete knowledge. We have rather to ask what great general principles has it established on so secure a basis that they meet with universal acceptance at the hands of competent zoologists?

It has doubtless been the object of morphology during the past half-century to illustrate and confirm the Darwinian theory. How far has it been successful? To answer this question we have to be sure of what we mean when we speak of the Darwinian theory. I think that we mean at least two things. (1) That the assemblage of animal forms as we now see them, with all their diversities of form, habit and structure, is directly descended from a precedent and somewhat different assemblage, and these in turn from a precedent and more different assemblage, and so on down to remote periods of geological time. Further, that throughout all these periods inheritance combined with changeability of structure have been the factors operative in producing the differences between the successive assemblages. (2) That the modifications of form which this theory of evolution implies have been rejected or preserved and accumulated by the action of natural selection.

As regards the first of these propositions, I think there can be no doubt that morphology has done great service in establishing our belief on a secure basis. The transmutation of animal forms in past time can not be proved directly; it can only be shown that, as a theory, it has a much higher degree of probability than any other that can be brought forward, and in order to establish the highest possible degree of probability, it was necessary to demonstrate that all anatomical, embryological and paleontological facts

were consistent with it. We are apt to forget, nowadays, that there is no *a priori* reason for regarding the resemblances and differences that we observe in organic forms as something different in kind from the analogous series of resemblances and differences that obtain in inanimate objects. This was clearly pointed out by Fleeming Jenkin in a very able and much-referred to article in the *North British Review* for June, 1867, and his argument from the *a priori* standpoint has as much force to-day as when it was written forty-three years ago. But it has lost almost all its force through the arguments *a posteriori* supplied by morphological science. Our belief in the transmutation of animal organization in past time is founded very largely upon our minute and intimate knowledge of the manifold relations of structural form that obtain among adult animals; on our precise knowledge of the steps by which these adult relations are established during the development of different kinds of animals; on our constantly increasing knowledge of the succession of animal forms in past time; and, generally, on the conviction that all the diverse forms of tissues, organs and entire animals are but the expression of an infinite number of variations of a single theme, that theme being cell-division, multiplication and differentiation. This conviction grew but slowly in men's minds. It was opposed to the cherished beliefs of centuries, and morphology rendered a necessary service when it spent all those years which have been described as "years in the wilderness" in accumulating such a mass of circumstantial evidence in favor of an evolutionary explanation of the order of animate nature as to place the doctrine of descent with modification on a secure foundation of fact. I do not believe that this foundation could have been so securely laid in any other way, and I hold that zoologists were actuated by a sound instinct in working so largely on morphological lines for forty years after Darwin wrote. For there was a large mass of fact and theory to be remodelled and brought into harmony with the new ideas, and a still larger vein of undiscovered fact to explore. The matter was difficult and the pace could not be forced. Morphology, therefore, deserves the credit of having done well in the past: the question remains, What can it do in the future?

It is evident, I think, that it can not do much in the way of adding new truths and general principles to zoological science, nor even much more that is useful in the verification of established principles, without enlarging its scope and methods. Hitherto—or, at any rate, until very recently—it has accepted certain guiding principles on faith, and, without inquiring too closely into their validity, has occupied itself with showing that, on the assumption that these principles are true, the phenomena of animal structure, development and succession receive a reasonable explanation.

We have seen that the fundamental principles relied upon during the last fifty years have been inheritance and variation. In every inference drawn from the comparison of one kind of animal structure with another, the morphologist founds himself on the assumption that different degrees of similitude correspond more or less closely to degrees of blood-relationship, and to-day there are probably few persons who doubt that this assumption is valid. But we must not forget that, before the publication of the "Origin of Species," it was rejected by the most influential zoologists as an idle speculation, and that it is imperilled by Mendelian experiments showing that characters may be split up

and reunited in different combinations in the course of a few generations. We do not doubt the importance of the principle of inheritance, but we are not quite so sure as we were that close resemblances are due to close kinship and remoter resemblances to remoter kinship.

The principle of variation asserts that like does not beget exactly like, but something more or less different. For a long time morphologists did not inquire too closely into the question how these differences arose. They simply accepted it as a fact that they occur, and that they are of sufficient frequency and magnitude, and that a sufficient proportion of them lead in such directions that natural selection can take advantage of them. Difficulties and objections were raised, but morphology on the whole took little heed of them. Remaining steadfast in its adherence to the principles laid down by Darwin, it contented itself with piling up circumstantial evidence, and met objection and criticism with an ingenious apologetic. In brief, its labors have consisted in bringing fresh instances, and especially such instances as seemed unconformable, under the rules, and in perfecting a system of classification in illustration of the rules. It is obvious, however, that, although this kind of study is both useful and indispensable at a certain stage of scientific progress, it does not help us to form new rules, and fails altogether if the old rules are seriously called into question.

As a matter of fact, admitting that the old rules are valid, it has become increasingly evident that they are not sufficient. Until a few years ago morphologists were open to the reproach that, while they studied form in all its variety and detail, they occupied themselves too little—if, indeed, they could be said to occupy themselves at all—with the question of how form is produced, and how, when certain forms are established, they are caused to undergo change and give rise to fresh forms. As Klebs has pointed out, the forms of animals and plants were regarded as the expression of their inscrutable inner nature, and the stages passed through in the development of the individual were represented as the outcome of purely internal and hidden laws. This defect seems to have been more distinctly realized by botanical than by zoological morphologists, for Hofmeister, as long ago as 1868, wrote that the most pressing and immediate aim of the investigator was to discover to what extent external forces acting on the organism are of importance in determining its form.

If morphology was to be anything more than a descriptive science, if it was to progress any further in the discovery of the relations of cause and effect, it was clear that it must alter its methods and follow the course indicated by Hofmeister. And I submit that an inquiry into the causes which produce alteration of form is as much the province of, and is as fitly called, morphology as, let us say, a discussion of the significance of the patterns of the molar teeth of mammals or a disputation about the origin of the cœlomic cavities of vertebrated and invertebrated animals.

There remains, therefore, a large field for morphology to explore. Exploration has begun from several sides, and in some quarters has made substantial progress. It will be of interest to consider how much progress has been made along certain lines of research—we can not now follow all the lines—and to forecast, if possible, the direction that this pioneer work will give to the morphology of the future.

I am not aware that morphologists have, until quite recently, had any very clear

concept of what may be expected to underlie form and structure. Dealing, as they have dealt, almost exclusively with things that can be seen or rendered visible by the microscope, they have acquired the habit of thinking of the organism as made up of organs, the organs of tissues, the tissues of cells, and the cells as made up—of what? Of vital units of a lower order, as several very distinguished biologists would have us believe; of physiological units, of micellæ, of determinants and biophors, or of pangenes; all of them essentially morphological conceptions; the products of imagination projected beyond the confines of the visible, yet always restrained by having only one source of experience—namely, the visible. One may give unstinted admiration to the brilliancy, and even set a high value on the usefulness, of these attempts to give formal representations of the genesis of organic structure, and yet recognize that their chief utility has been to make us realize more clearly the problems that have yet to be solved.

Stripped of all the verbiage that has accumulated about them, the simple questions that lie immediately before us are: What are the causes which produce changes in the forms of animals and plants? Are they purely internal, and, if so, are their laws discoverable? Or are they partly or wholly external, and, if so, how far can we find relations of cause and effect between ascertained chemical and physical phenomena and the structural responses of living beings?

As an attempt to answer the last of these questions, we have the recent researches of the experimental morphologists and embryologists directed towards the very aim that Hofmeister proposed. Originally founded by Roux, the school of experimental embryology has outgrown its infancy and has developed into a vigorous youth. It has produced some very remarkable results, which cannot fail to exercise a lasting influence on the course of zoological studies. We have learned from it a number of positive facts, from which we may draw very important conclusions, subversive of some of the most cherished ideas of whilom morphologists. It has been proved by experiment that very small changes in the chemical and physical environment may and do produce specific form-changes in developing organisms, and in such experiments the consequence follows so regularly on the antecedent that we can not doubt that we have true relations of cause and effect. It is not the least interesting outcome of these experiments that, as Loeb has remarked, it is as yet impossible to connect in a rational way the effects produced with the causes which produced them, and it is also impossible to define in a simple way the character of the change so produced. For example, there is no obvious connection between the minute quantity of sulphates present in sea-water and the number and position of the characteristic calcareous spicules in the larva of a sea-urchin. Yet Herbst has shown that if the eggs of sea-urchins are reared in sea-water deprived of the needful sulphates (normally .26 per cent. magnesium sulphate and .1 per cent. calcium sulphate), the number and relative positions of these spicules are altered, and, in addition, changes are produced in other organs, such as the gut and the ciliated bands. Again, there is no obvious connection between the presence of a small excess of magnesium chloride in sea-water and the development of the paired optic vesicles. Yet Stockard, by adding magnesium chloride to sea-water in the proportion of 6 grams of the former to 100 c.c. of the latter, has produced specific effects on the eyes of developing embryos of the minnow *Fundulus*

heteroclitus: the optic vesicles, instead of being formed as a widely separated pair, were caused to approach the median line, and in about 50 per cent. of the embryos experimented upon the changes were so profound as to give rise to cyclopean monsters. Many other instances might be cited of definite effects of physical and chemical agencies on particular organs, and we are now forced to admit that inherited tendencies may be completely overcome by a minimal change in the environment. The nature of the organism, therefore, is not all important, since it yields readily to influences which at one time we should have thought inadequate to produce perceptible changes in it.

It is open to any one to argue that, interesting as experiments of this kind may be, they throw no light on the origin of permanent—that is to say, inheritable—modifications of structure. It has for a long time been a matter of common knowledge that individual plants and animals react to their environment, but the modifications induced by these reactions are somatic; the germ-plasm is not affected, therefore the changes are not inherited, and no permanent effect is produced in the characters of the race or species. It is true that no evidence has yet been produced to show that form-changes as profound as those that I have mentioned are transmitted to the offspring. So far the experimenters have not been able to rear the modified organisms beyond the larval stages, and so there are no offspring to show whether cyclopean eyes or modified forms of spicules are inherited or not. Indeed, it is possible that the balance of organization of animals thus modified has been upset to such an extent that they are incapable of growing into adults and reproducing their kind.

But evidence is beginning to accumulate which shows that external conditions may produce changes in the germ-cells as well as in the soma, and that such changes may be specific and of the same kind as similarly produced somatic changes. Further, there is evidence that such germinal changes are inherited—and, indeed, we should expect them to be, because they are germinal.

The evidence on this subject is as yet meager, but it is of good quality and comes from more than one source.

There are the well-known experiments of Weismann, Standfuss, Merrifield and E. Fischer on the modification of the color patterns on the wings of various lepidoptera.

In the more northern forms of the fire-butterfly, *Chrysophanus* (*Polyommatus*) *phlœas,* the upper surfaces of the wings are of a bright red-gold or copper color with a narrow black margin, but in southern Europe the black tends to extend over the whole surface of the wing and may nearly obliterate the red-gold color. By exposing pupæ of caterpillars collected at Naples to a temperature of 10° C. Weismann obtained butterflies more golden than the Neapolitan, but blacker than the ordinary German race, and conversely, by exposing pupæ of the German variety to a temperature of about 38° C., butterflies were obtained blacker than the German, but not so black as the Neapolitan variety. Similar deviations from the normal standard have been obtained by like means in various species of *Vanessa* by Standfuss and Merrifield. Standfuss, working with the small tortoise-shell butterfly (*Vanessa urticæ*), produced color aberrations by subjecting the pupæ to cold, and found that some specimens reared under normal conditions from the eggs produced by the aberrant forms exhibited the same aberrations, but in a lesser degree. Weis-

mann obtained similar results with the same species. E. Fischer obtained parallel results with *Arctia caja*, a brightly colored diurnal moth of the family Bombycidæ. Pupæ of this moth were exposed to a temperature of 8° C., and some of the butterflies that emerged were very dark-colored aberrant forms. A pair of these dark aberrants were mated, and the female produced eggs, and from these larvæ and pupæ were reared at a normal temperature. The progeny was for the most part normal, but some few individuals exhibited the dark color of the parents, though in a less degree. The simple conclusions to be drawn from the results of these experiments is that a proportion of the germ-cells of the animals experimented upon were affected by the abnormal temperatures, and that the reaction of the germ-cells was of the same kind as the reaction of the somatic cells and produced similar results. As everybody knows, Weismann, while admitting that the germ-cells were affected, would not admit the simple explanation, but gave another complicated and, in my opinion, wholly unsupported explanation of the phenomena.

In any case this series of experiments was on too small a scale, and the separate experiments were not sufficiently carefully planned to exclude the possibility of error. But no objection of this kind can be urged against the careful and prolonged studies of Tower on the evolution of chrysomelid beetles of the genus *Leptinotarsa*. *Leptinotarsa*—better known, perhaps, by the name *Doryphora*—is the potato-beetle, which has spread from a center in north Mexico southwards into the isthmus of Panama and northwards over a great part of the United States. It is divisible into a large number of species, some of which are dominant and widely ranging; others are restricted to very small localities. The specific characters relied upon are chiefly referable to the coloration and color patterns of the epicranium, pronotum, elytra and underside of the abdominal segments. In some species the specific markings are very constant, in others, particularly in the common and wide-ranging *L. decemlineata*, they vary to an extreme degree. As the potato-beetle is easily reared and maintained in captivity, and produces two broods every year, it is a particularly favorable subject for experimental investigation. Tower's experiments have extended over a period of eleven years, and he has made a thorough study of the geographical distribution, dispersal, habits and natural history of the genus. The whole work appears to have been carried out with the most scrupulous regard to scientific accuracy, and the author is unusually cautious in drawing conclusions and chary of offering hypothetical explanations of his results. I have been greatly impressed by the large scale on which the experiments have been conducted, by the methods used, by the care taken to verify every result obtained, and by the great theoretical importance of Tower's conclusions. I can do no more now than allude to some of the most remarkable of them.

After showing that there are good grounds for believing that color production in insects is dependent on the action of a group of closely related enzymes, of which chitase, the agent which produces hardening of chitin, is the most important, Tower demonstrates by a series of well-planned experiments that colors are directly modified by the action of external agencies — viz., temperature, humidity, food, altitude and light. Food chiefly affects the subhypodermal colors of the larvæ, and does not enter much into account; the most important agents affecting the adult coloration being temperature and

humidity. A *slight* increase or a *slight* decrease of temperature or humidity was found to stimulate the action of the color-producing enzymes, giving a tendency to melanism; but a large increase or decrease of temperature or humidity was found to inhibit the action of the enzymes, producing a strong tendency to albinism.

A set of experiments was undertaken to test the question whether coloration changes induced by changed environmental conditions were inherited, increased or dropped in successive generations. These experiments, carried on for ten lineal generations, showed that the changed conditions immediately produced their maximum effect; that they were purely somatic and were not inherited, the progeny of individuals which had been exposed to changed conditions through several generations promptly reverting when returned to normal conditions of environment. So far the results are confirmatory of the well-established proposition that induced somatic changes are not inheritable.

But it was found necessary to remove the individuals experimented upon from the influence of changed conditions during the periods of growth and maturation of the germ-cells. Potato-beetles emerge from the pupa or from hibernation with the germ-cells in an undeveloped condition, and the ova do not all undergo their development at once, but are matured in batches. The first batch matures during the first few days following emergence, then follows an interval of from four to ten days, after which the next batch of eggs is matured, and so on. This fact made it possible to test the effect of altered conditions on the maturing germ-cells by subjecting its imagos to experimental conditions during the development of some of the batches of ova and to normal conditions during the development of other batches.

In one of the experiments four male and four female individuals of *L. decemlineata* were subjected to very hot and dry conditions, accompanied by low atmospheric pressure, during the development and fertilization of the first three batches of eggs. Such conditions had been found productive of albinic deviations in previous experiments. As soon as the eggs were laid they were removed to normal conditions, and the larvæ and pupæ reared from them were kept in normal conditions. Ninety-eight adult beetles were reared from these batches of eggs, of which eighty-two exhibited the characters of an albinic variety found in nature and described as a species under the name *pallida;* two exhibited the characters of another albinic species named *immaculothorax,* and fourteen were unmodified *decemlineatas.* This gave a clear indication that the altered conditions had produced modifications in the germ-cells which were expressed by color changes in the adult individuals reared from them. To prove that the deviations were not inherent in the germ-plasm of the parents, the latter were kept under normal conditions during the periods of development and fertilization of the last two batches of eggs; the larvæ and pupæ reared from these eggs were similarly subjected to normal conditions, and gave rise to sixty-one unmodified *decemlineatas,* which, when bred together, came true to type for three generations. The *decemlineata* forms produced under experimental conditions also came true to type when bred together. Of the *pallida* forms produced by experimental conditions all but two males were killed by a bacterial disease. These two were crossed with normal *decemlineata* females, and the result was a typical Mendelian segregation, as shown by the following table:

2 ♂ *pallida* × 2 ♀ *decemlineata*

Hybrids, *decemlineata* characters dominant

5 ♂ × 3 ♀

Pallida 6 ♂, 4 ♀ — *Decemlineata* and hybrids 16 ♂, 14 ♀

Pallida 18 ♂, 23 ♀ — *Pallida* 8 ♂, 7 ♀ — *Decemlineata* and hybrids 26 ♂, 28 ♀

This is a much more detailed experiment than those of Standfuss, Merrifield and Fisher, and it shows that the changes produced by the action of altered conditions on the maturing germ-cells were definite and discontinuous, and therefore of the nature of mutations in De Vries's sense.

In another experiment Tower reared three generations of *decemlineata* to test the purity of his stock. He found that they showed no tendency to produce extreme variations under normal conditions. From this pure stock seven males and seven females were chosen and subjected during the maturation periods of the first two batches of ova to hot and dry conditions. Four hundred and nine eggs were laid, from which sixty-nine adults were reared, constituted as follows:

Twenty (12 ♂, 8 ♀) apparently normal *decemlineata.*

Twenty-three (10 ♂, 13 ♀) *pallida.*

Five (2 ♂, 3 ♀) *immaculothorax.*

Sixteen (9 ♂, 7 ♀) *albida.*

These constituted lot A.

The same seven pairs of parents subjected during the second half of the reproductive period to normal conditions gave 840 eggs, from which were reared 123 adults, all *decemlineatas.* These constituted lot B. The *decemlineatas* of lot A and lot B were reared side by side under normal and exactly similar conditions. The results were striking. From lot B normal progeny were reared up to the tenth generation, and, as usual in the genus, two generations were produced in each year. The *decemlineatas* of lot A segregated into two lots in the second generation. A^1 were normal in all respects, but A^2, while retaining the normal appearance of *decemlineata,* went through five generations in a year, and this for three successive years, thus exhibiting a remarkable physiological modification, and one without parallel in nature, for no species of the genus *Leptinotarsa* are known which produce more than two generations in the year. This experiment is a sufficient refutation of Weismann's argument that the inheritance of induced modifications in *Vanessa urticæ* is only apparent, the phenomena observed being due to the inheritance of two kinds of determinants—one from dark-colored forms which are phyletically the oldest and the other from more gaily colored forms derived from the darker forms. There is no evidence whatever that there was ever a species or variety of potato-beetle that produced more than two, or at the most, and then as an exception, three broods in a year.

The modified albinic forms in this last experiment of Tower's were weakly; they were bred through two or three generations

and came true to type, but then died out. No hybridization experiments were made with them, but in other similar experiments, which I have not time to mention in detail, modified forms produced by the action of changed conditions gave typical Mendelian characters when crossed with unmodified *decemlineatas,* thus proving that the induced characters were constant and heritable according to the regular laws.

I have thought it worth while to relate these experiments at some length, because they seem to me to be very important, and because they do not appear to have attracted the attention in this country that they deserve.

They are confirmed to a very large extent by the experiments of Professor Klebs on plants, the results of which were published this summer in the Croonian Lecture on "Alterations of the Development and Forms of Plants as a Result of Environment." As I have only a short abstract of the Croonian Lecture to refer to, I can not say much on this subject for fear of misrepresenting the author; but, as far as I can judge, his results are quite consistent with those of Tower. *Sempervivum funckii* and *S. acuminatum* were subjected to altered conditions of light and nutrition, with the result that striking variations, such as the transformation of sepals into petals, of petals into stamens, of stamens into petals and into carpels, were produced. Experiments were made on *Sempervivum acuminatum,* with the view of answering the question whether such alterations of flowers can be transmitted. The answer was in the affirmative. The seeds of flowers artificially altered and self-fertilized gave rise to twenty-one seedlings, among which four showed surprising deviations of floral structure. In two of these seedlings all the flowers were greatly altered, and presented some of the modifications of the mother plant, especially the transformation of stamens into petals. These experiments are still in progress, and it would perhaps be premature to lay too much stress upon them if it were not for the fact that they are so completely confirmatory of the results obtained by similar methods in the animal kingdom.

I submit to you that evidence is forthcoming that external conditions may give rise to inheritable alterations of structure. Not, however, as was once supposed, by producing specific changes in the parental soma, which changes were reflected, so to speak, upon the germ-cells. The new evidence confirms the distinctions drawn by Weismann between somatic and germinal variations. It shows that the former are not inherited, while the latter are; but it indicates that the germ may be caused to vary by the action of external conditions in such a manner as to produce specific changes in the progeny resulting from it. It is no more possible at the present time to connect rationally the action of external conditions on the germ-cells with the specific results produced in the progeny than it is possible to connect cause with effect in the experiments of Herbst and Stockard; but, when we compare these two kinds of experiments, we are no longer able to argue that it is inconceivable that such and such conditions acting on the germplasm can produce such and such effects in the next generation of adults. We must accept the evidence that things which appeared inconceivable do in fact happen, and in accepting this we remove a great obstacle from the path of our inquiries, and gain a distinct step in our attempts to discover the laws which determine the production of organic form and structure.

But such experiments as those which I have mentioned only deal with one aspect of the problem. They tell us about ex-

ternal conditions and the effects that they are observed to produce upon the organism. They give us no definite information about the internal changes which, taken together, constitute the response of the organism to external stimuli. As Darwin wrote, there are two factors to be taken into account—the nature of the conditions and the nature of the organisms—and the latter is much the more important of the two. More important because the reactions of animals and plants are manifold; but, on the whole, the changes in the conditions are few and small in amount. Morphology has not succeeded in giving us any positive knowledge of the nature of the organism, and in this matter we must turn for guidance to the physiologists, and ask of them how far recent researches have resulted in the discovery of factors competent to account for change of structure. Perhaps the first step in this inquiry is to ask whether there is any evidence of internal chemical changes analogous in their operation to the external physical and chemical changes which we have been dealing with.

There is a great deal of evidence, but it is extremely difficult to bring it to a focus and to show its relevancy to the particular problems that perplex the zoologist. Moreover, the evidence is of so many different kinds, and each kind is so technical and complex, that it would be absurd to attempt to deal with it at the end of an address that has already been drawn out to sufficient length. But perhaps I may be allowed to allude to one or two generalizations which appear to me to be most suggestive.

We shall all agree that, at the bottom, production and change of form is due to increase or diminution of the activities of groups of cells, and we are aware that in the higher animals change of structure is not altogether a local affair, but carries with it certain consequences in the nature of correlated changes in other parts of the body. If we are to make any progress in the study of morphogeny, we ought to have as exact ideas as possible as to what we mean when we speak of the activities of cells and of correlation. On these subjects physiology supplies us with ideas much more exact than those derived from morphology.

It is, perhaps, too sweeping a generalization to assert that the life of any given animal is the expression of the sum of the activities of the enzymes contained in it, but it seems well established that the activities of cells are, if not wholly, at all events largely, the result of the actions of the various kinds of enzymes held in combination by their living protoplasm. These enzymes are highly susceptible to the influence of physical and chemical media, and it is because of this susceptibility that the organism responds to changes in the environment, as is clearly illustrated in a particular case by Tower's experiments on the production of color changes in potato-beetles. Bayliss and Starling have shown that in lower animals, protozoa and sponges, in which no nervous system has been developed, the response of the organism to the environment is effected by purely chemical means. In protozoa, because of their small size, the question of coadaptation of function hardly comes into question; but in sponges, many of which are of large size, the mechanism of coadaptation must also be almost exclusively chemical. Thus we learn that the simplest and, by inference, the phyletically oldest mechanism of reaction and coordination is a chemical mechanism. In higher animals the necessity for rapid reaction to external and internal stimuli has led to the development of a central and peripheral nervous

system, and as we ascend the scale of organization, this assumes a greater and greater importance as a coordinating bond between the various organs and tissues of the body. But the more primitive chemical bond persists, and is scarcely diminished in importance, but only overshadowed by the more easily recognizable reactions due to the working of the nervous system. In higher animals we may recognize special chemical means whereby chemical coadaptations are established and maintained at a normal level, or under certain circumstances altered. These are the internal secretions produced by sundry organs, whether by typical secretory glands (in which case the internal secretion is something additional and different from the external secretion), or by the so-called ductless glands, such as the thyroid, the thymus, the adrenal bodies, or by organs which can not strictly be called glands—namely, the ovaries and testes. All these produce chemical substances which, passing into the blood or lymph, are distributed through the system, and have the peculiar property of regulating or exciting the specific functions of other organs. Not, however, of all the organs, for the different internal secretions are more or less limited and local in their effects: one affecting the activity of this and another the activity of that kind of tissue or organ. Starling proposed the name hormones for the internal secretions because of their excitatory properties (ὁρμάω, to stir up, to excite).

Hormones have been studied chiefly from the point of view of their stimulating effect on the metabolism of various organs. From the morphologist's point of view, interest chiefly attaches to the possibility of their regulating and promoting the production of form. It might be expected that they should be efficient agents in regulating form, for, if changes in structure are the result of the activities of groups of cells, and the activities of cells are the results of the activities of the enzymes which they contain, and if the activities of the enzymes are regulated by the hormones, it follows that the last-named must be the ultimate agents in the production of form. It is difficult to obtain distinct evidence of this agency, but in some cases at least the evidence is sufficiently clear. I will confine myself to the effects of the hormones produced by the testes and ovaries. These have been proved to be intimately connected with the development of secondary sexual characters—such, for instance, as the characteristic shape and size of the horns of the bull; the comb, wattles, spurs, plumage color and spurs in poultry; the swelling on the index finger of the male frog; the shape and size of the abdominal segments of crabs. These are essentially morphological characters, the results of increased local activity of cell-growth and differentiation. As they are attributable to the stimulating effect of the hormone produced by the male organ in each species, they afford at least one good instance of the production of a specific change of form as the result of an internal chemical stimulus. We get here a hint as to the nature of the chemical mechanism which excites and correlates form and function in higher organisms; and, from what has just been said, we perceive that this is the most primitive of all the animal mechanisms. I submit that this is a step towards forming a clear and concrete idea of the inner nature of the organism. There is one point, and that a very important one, upon which we are by no means clear. We do not know how far the hormones themselves are liable to change, whether by the action of external conditions or by the reciprocal action of the ac-

tivities of the organs to which they are related. It is at least conceivable that agencies which produce chemical disturbances in the circulating fluids may alter the chemical constitution of the hormones, and thus produce far-reaching effects. The pathology of the thyroid gland gives some ground for belief that such changes may be produced by the action of external conditions. But, however this may be, the line of reasoning that we have followed raises the expectation that a chemical bond must exist between the functionally active organs of the body and the germ-cells. For if, in the absence of a specialized nervous system, the only possible regulating and coadapting mechanism is a chemical mechanism, and if the specific activities of a cell are dependent on the enzymes which it holds in combination, the germ-cells of any given animal must be the depository of a stock of enzymes sufficient to insure the due succession of all its developmental stages as well as of its adult structure and functions. And as the number of blastomeres increases, and the need for coordination of form and function arises, before ever the rudiments of a nervous system are differentiated, it is necessary to assume that there is also a stock of appropriate hormones to supply the chemical nexus between the different parts of the embryo. The only alternative is to suppose that they are synthesized as required in the course of development. There are grave objections to this supposition. All the evidence at our disposal goes to show that the potentialities of germ-cells are determined at the close of the maturation divisions. Following the physiological line of argument, it must be allowed that in this connection "potentiality" can mean nothing else than chemical constitution. If we admit this, we admit the validity of the theory advanced by more than one physiologist that heritable "characters" or "tendencies" must be identified with the enzymes carried in the germ-cells. If this be a true representation of the facts, and if the most fundamental and primitive bond between one part of an organism and another is a chemical bond, it can hardly be the case that germ-cells—which, *inter alia*, are the most primitive, in the sense of being the least differentiated, cells in the body—should be the only cells which are exempt from the chemical influences which go to make up the coordinate life of the organism. It would seem, therefore, that there is some theoretical justification for the inheritance of induced modifications, provided that these are of such a kind as to react chemically on the enzymes contained in the germ-cells.

One further idea that suggests itself to me and I have done. Is it possible that different kinds of enzymes exercise an inhibiting influence on one another; that germ-cells are "undifferentiated" because they contain a large number of enzymes, none of which can show their activities in the presence of others, and that what we call "differentiation" consists in the segregation of the different kinds into separate cells, or perhaps, prior to cell-formation, into different parts of the fertilized ovum, giving rise to the phenomenon known to us as prelocalization? The idea is purely speculative; but, if it could be shown to have any warrant, it would go far to assist us in getting an understanding of the laws of the production of form.

I have been wandering in territories outside my own province, and I shall certainly be told that I have lost my way. But my thesis has been that morphology, if it is to make useful progress, must come out of its reserves and explore new ground. To explore is to tread unknown paths, and one is likely to lose one's way in the unknown.

To stay at home in the environment of familiar ideas is no doubt a safe course, but it does not make for advancement. Morphology, I believe, has as great a future before it as it has a past behind it, but it can only realize that future by leaving its old home, with all its comfortable furniture of well-worn rules and methods, and embarking on a journey, the first stages of which will certainly be uncomfortable and the end is far to seek.

G. C. BOURNE

GEOGRAPHY AND SOME OF ITS MORE PRESSING NEEDS[1]

AT the close of a reign which has practically coincided with the first decade of a new century, it is natural to look back and summarize the progress of geography during the decade. At the beginning of a new reign it is equally natural to consider the future. Our new sovereign is one of the most traveled of men. No monarch knows the world as he knows it; no monarch has ruled over a larger empire or seen more of his dominions. His advice has been to wake up, to consider and to act. It will be in consonance with this advice if I pay more attention to the geography of the future than to that of the past, and say more about its applications than about its origins.

Yet I do so with some reluctance, for the last decade has been one of the most active and interesting in the history of our science. The measurement of new and the remeasurement of old arcs will give us better data for determining the size and shape of the earth. Surveys of all kinds, from the simple route sketches of the traveler to the elaborate cadastral surveys of some of the more populous and settled regions have so extended our knowledge of the surface features of the earth that a map on the scale of 1,000,000 is not merely planned, but actually partly executed. Such surveys and such maps are the indispensable basis of our science, and I might say much about the need for accurate topographical surveys. This, however, has been done very fully by some of my predecessors in this chair in recent years.

The progress of oceanography has also been great. The soundings of our own and other admiralties, of scientific oceanographical expeditions, and those made for the purpose of laying cables, have given us much more detailed knowledge of the irregularities of the ocean floor. An international map of oceanic contours due to the inspiration and munificence of the prince of oceanographers and of Monaco has been issued during the decade, and so much new material has accumulated that it is now being revised. A comparison of the old and new editions of Krümmel's "Ozeanographie" shows us the immense advances in this subject.

Great progress has been made on the geographical side of meteorology and climate. The importance of this knowledge for tropical agriculture and hygiene has led to an increase of meteorological stations all over the hot belt—the results of which will be of immense value to the geographer. Mr. Bartholomew's "Atlas of Meteorology" appeared at the beginning, and Sir John Eliot's "Meteorological Atlas of India" at the end of the decade. Dr. Hann's "Lehrbuch" and the new edition of his "Climatology," Messrs. Hildebrandsson and Teisserenc de Bort's great work, "The Study of the Upper Atmosphere," are among the landmarks of progress. The record is marred only by the closing of Ben Nevis Observatory. A comparison of the present number and dis-

[1] Address to the Geographical Section of the British Association for the Advancement of Science, Sheffield, 1910.

tribution of meteorological stations with those given in Bartholomew's atlas would show how great the extension of this work is.

I have not time to recapitulate the innumerable studies of geographical value issued by many meteorological services, observatories and observers—public and private—but I may call attention to the improved weather maps and to the excellent pilot charts of the North Atlantic and of the Indian Ocean published monthly by our Meteorological Office.

Lake studies have also been a feature of this decade, and none are so complete or so valuable as the Scottish Lakes Survey—a work of national importance, undertaken by private enthusiasm and generosity. We have to congratulate Sir John Murray and Mr. Pullar on the completion of a great work.

In geology I might note that we now possess a map of Europe on a scale of 1:1,500,000 prepared by international cooperation and also one of North America on a smaller scale. The thanks and congratulations of all geographers are due to Professor Suess on the conclusion of his classical study of the face of the earth, the first comprehensive study of the main divisions and characteristics of its skeleton. English readers are indebted to Professor and Miss Sollas for the brilliant English translation which they have prepared.

A new movement, inspired mainly by Professor Flahault in France, Professor Geddes in this country, Professors Engler, Drude and Schimper in Germany has arisen among botanists, and at last we have some modern botanical geography which is really valuable to the geographer. I wish we could report similar progress in zoological geography, but that, I trust, will come in the next decade.

I pass over the various expensive arbitrations and commissions to settle boundary disputes which have in many cases been due to geographical ignorance, also the important and fascinating problems of the growth of our knowledge of the distribution of economic products and powers existing and potential, and the new geographical problems for statesmen due to the industrial revolution in Japan and China.

It is quite impossible to deal with the exploration of the decade. Even in the past two years we have had Peary and Shackleton, Stein and Hedin, the Duke of the Abruzzi, and a host of others returning to tell us of unknown or little known parts of the globe. We hope to hear some of the results of latest investigations from Dr. Charcot.

We wish success to Scott and his companions, to Bruce, Amundsen, Filchner, and others, British, American, German, or whatever nationality, who go to the south or north polar ice worlds, to Longstaff, Bruce, and others exploring the Himalayan regions, and to other geographical expeditions too numerous to mention.

One word of caution may, perhaps, be permitted. There is a tendency on the part of the public to confuse geographical exploration and sport. The newspaper reporter naturally lays stress on the unusual in any expedition, the accidental rather than the essential, and those of us who have to examine the work of expeditions know how some have been unduly boomed because of some adventurous element, while others have not received adequate popular recognition because all went well. The fact that all went well is in itself a proof of competent organization. There is no excuse for us in this section if we fall into the journalist's mistake, and we shall certainly be acting against the interests of

both our science and our section if we do so.

It was not my intention in this address to raise the question of what is geography, but various circumstances make it desirable to say a few words upon it. We are all the victims of the geographical teaching of our youth, and it is easy to understand how those who have retained unchanged the conceptions of geography they gained at school many years ago cavil at the recognition of geography as a branch of science. Moreover the geography of the schools still colors the conceptions of some geographers who have nevertheless done much to make school geography scientific and educational. Many definitions of geography are consequently too much limited by the arbitrary but traditional division of school subjects. In schools tradition and practical convenience have on the whole rightly determined the scope of the different subjects. Geography in schools is best defined as the study of the earth as the home of man; its limits should not be too closely scrutinized, and it should be used freely as a coordinating subject.

The present division into sections of the British association is also largely a matter of practical convenience, but we are told that the present illogical arrangement of sections distresses some minds. No doubt there are some curious anomalies. The most glaring, perhaps, is that of combining mathematics with physics—as if mathematical methods were used in no other subjects.

There is a universal tendency to subdivision and an ever-increasing specialism, but there is also an ever-growing interdependence of different parts of science which the British Association is unquestionably bound to take into account. At present this is chiefly done by joint meetings of sections, a wise course, of which this section has been one of the chief promoters. It is possible that some more systematic grouping of sections might be well advised, but such a reform should be systematic and not piecemeal. It is one which raises the whole question of the classification of knowledge. This is so vast a problem and one on which such divergent opinions are held that I must apologize for venturing to put forward some tentative suggestions.

It might be found desirable to take as primary divisions the mathematical, physical, biological, anthropological and geographical groups. Statistics might be regarded as a subdivision of mathematics or as a field common to mathematics and any of the other groups. In the second might be the subdivisions physics and chemistry. Each would devote a certain proportion of time to its applied aspects—or there might be subsections on physics, which would include engineering and applied chemistry. In the biological group there would be botany, zoology, in both cases including paleontology and embryology, and applied biology, which would be dealt with in one or other of the ways I have suggested, and would include agriculture, fisheries, etc. (Medicine we leave out at present.) In the psychological group there would be a new section on psychology, with the education section as the practical appendage. Mathematical application would be considered in each of the other sections which use mathematical notations. In the anthropological group there would be the present anthropology and theoretical economics with applied economics and administration. In the geographical group there would be geography and geology, the practical applications of geography being considered in joint meetings, or subsections—for instance, geography and physics for questions of atmospheric and oceanic circula-

tion, geography and economics for questions of transportation, etc.

So much, then, for the classification of geography with reference to the other sciences. I should like to say a few words about geographical classification and geographical terminology.

In the scheme of the universe it is possible to consider the earth as a unit, with its own constitution and history. It has an individuality of its own, though for the astronomer it is only one example of a particular type of heavenly bodies. As geographers we take it as our unit individual in the same way that an anatomist takes a man. We see that it is composed of different parts and we try to discover what these are, of what they are composed, what their function is, what has been their history.

The fundamental division is into land, water and air. Each has its forms and its movements. The forms are more obvious and persistent in the land. They are least so in the atmosphere, though forms exist—some of which are at times made visible by clouds, and many can be clearly discerned on isobaric charts. The land is the temporarily permanent; the water and atmosphere the persistently mobile; the latter more so than the former. The stable forms of the land help to control the distribution and movements of the waters and to a less extent those of the atmosphere. How great the influence of the distribution of land and water is on the atmosphere may be seen in the monsoon region of eastern Asia.

We can analyze and classify the subdivision of the land, the water, and the atmosphere. Each has given rise to a special branch of study.

Geomorphology deals with the forms of the land and their shaping—geomorphology, oceanography and climatology. Three things have to be kept clearly in view: (i) the structure, including the composition, of the more permanent substance of the form; (ii) the forces which are modifying it; and (iii) the phase in the cycle of forms characteristic of such structure acted on by such forms. We may say that any form is a function of structure, process and time. The matter is even more complicated, for we have instances, *e. g.*, in antecedent drainage systems, of the conditions of a previous cycle affecting a subsequent one—a kind of heredity of forms which can not be neglected.

The geomorphologist is seeking for a genetic classification of forms, and in the works of Davis, Penck, Richtofen and Supan and their pupils are being accumulated the materials for a more complete and systematic classification of forms. As you all know, the question of terms for the manifold land-forms is a difficult one and apt to engender much more controversy than the analysis of the forms themselves. I have long thought that we shall be driven to some notation analogous to that of the chemists. I have not yet had time to work such a notation out in detail, but it might take the form of using different symbols for the three factors noted above —say, letters for different kinds of structure, and, say, Arabic figures for processes, and Roman figures for the stage of a cycle the form has reached.

Take a very simple set of structures and indicate each by a letter:

Structure	homogeneous		A	A′
	layered	horizontal	B	B′
		tilted	C	C′
		folded	D	D′
	mixed		E	E′

If pervious or impervious, a *p* or an *i* could be added—*e. g.*, a tilted limestone with faults would be $C'p$.

Next indicate the commoner erosion processes by Arabic numerals:

Process	surface water	1
	ice	2
	wind	3
	sea	4

One process may have followed another, *e. g.*, where a long period of ice erosion has been followed by water erosion we might write 21 where these alternate annually, say 2.1.

The phase of the cycle might be denoted by Roman figures. A scale of V. might be adopted and I., III. and V. used for youthful, middle-aged and old-aged, as Professor Davis calls them; or early, middle and late phases, as I shall prefer to term them. II. and IV. would denote intermediate stages.

A scarped limestone ridge in a relatively mature phase like the Cotswolds would be —if we put the process first—1 C^1 III.; a highland like the southern Uplands of Scotland would be denoted by the formula 1. 2.1 E^1 III.

This is the roughest suggestion, but it shows how we could label our cases of notes, and pigeon-hole our types of forms —and prevent for the present undue quarrelling over terms. No doubt there would be many discussions about the exact phase of the cycle, for example, whether ice in addition to water has been an agent in shaping this or that form. But, after all, these discussions would be more profitable than quarrels as to which descriptive term, or place name, or local usage should be adopted to distinguish it.

In the case of climatology, there is coming to be a general consensus of opinion as to what are the chief natural divisions, and the use of figures and letters to indicate them has been followed by several authors. This should also be attempted for oceanography.

If any international agreement of symbols and colors could be come to for such things it would be a great gain, and I hope to bring this matter before the next international geographical congress.

We have still to come to geography itself. What are the units smaller than the whole earth with which our science has to deal? When we fix our attention on parts of the earth, and ask what is a natural unit, we are hampered by preconceptions. We recognize species, or genera, families or races as units—but they are abstract rather than concrete units. Speaking for myself, I should say that every visible concrete natural unit on the earth's surface consisting of more than one organic individual is a geographical unit. It is a common difficulty not to be able to see the wood for the trees; it is still more difficult to recognize that the wood consists of more than trees, that it is a complex of trees and other vegetation, fixed to a definite part of the solid earth and bathed in air.

The family, the species and the race are abstract ideas. If we consider them as units, it is because they have a certain historical continuity. They have not an actual physical continuity as the component parts of an individual have. Concrete physical continuity is what differentiates the geographical unit. We may speak of a town or state as composed of people, but a complete conception of either must include the spacial connections which unite its parts. A town is not merely an association of individuals, nor is it simply a piece of land covered with streets and buildings; it is a combination of both.

In determining the greater geographical units, man need not be taken into account. We are too much influenced by the mobility of man, by his power to pass from one region to another, and we are apt to forget that his influence on his environment is

negligible except when we are dealing with relatively small units. The geographer will not neglect man; he will merely be careful to prevent himself from being unduly influenced by the human factor in selecting his major units.

Some geographers and many geologists have suggested that land forms alone need be taken into account in determining these geographical units. Every different recognizable land form is undoubtedly a geographical unit. A great mountain system, such as that of western North America, or a vast lowland, such as that which lies to the east of the Rocky Mountains, is undoubtedly a geographical unit of great importance, but its subdivisions are not wholly orographical. The shores of the Gulf of Mexico can not be considered as similar geographically to those of the Arctic Ocean, even if they are morphologically homologous. I wish to lay great stress on the significance of vegetation to the geographer for the purposes of regional classification. I do not wish to employ a biological terminology nor to raise false analogies between the individual organism and the larger units of which it is a part, but I think we should do well to consider what may be called the life or movement going on in our units as well as their form. We must consider the seasonal changes of its atmospheric and of its water movements, as well as the parts of the earth's crust which they move over and even slightly modify. For this purpose a study of climatic regions is as necessary as a study of morphological regions. The lowlands of the Arctic area are very different from those at or near the tropics. The rhythm of their life is different, and this difference is revealed in the differences of vegetation.

By vegetation I mean not the flora, the historically related elements, but the vegetable coating, the space-related elements. Vegetation in this sense is a geographical phenomenon of fundamental importance. It indicates quality—quality of atmosphere and quality of soil. It is a visible synthesis of the climatic and edaphic elements. Hence the vast lowlands of relatively uniform land features are properly divided into regions according to vegetation—tundra, pine forest, deciduous forest, warm evergreen forest, steppe and scrub. Such differences of vegetation are full of significance even in mountainous areas.

The search after geographical unity—after general features common to recognizable divisions of the earth's surface, the analysis of these, their classification into types, the comparisons between different examples of the types—seem to me among the first duties of a geographer. Two sets of maps are essential—topographical and vegetational—the first giving the superficial topography and as far as possible its surface irregularities, the latter indicating quality of climate and soil.

Much has been said in recent years—more particularly from this presidential chair—on the need for reliable topographical maps. Without such maps no others can be made. But when they are being made it would be very easy to have a general vegetational map compiled. Such maps are even more fundamental than geological maps, and they can be constructed more rapidly and cheaply. Every settled country, and more particularly every partially settled country, will find them invaluable if there is to be any intelligent and systematic utilization of the products of the country.

The geographer's task I am assuming is to study environments, to examine the forms and qualities of the earth's surface, and to recognize, define and classify the different kinds of natural units into which

it can be divided. For these we have not as yet even names. It may seem absurd that there should be this want of terms in a subject which is associated in the minds of most people with a superfluity of names. I have elsewhere suggested the use of the terms major natural region, natural region, district and locality to represent different grades of geographical units, and have also attempted to map the seventy or eighty major natural regions into which the earth's surface is divided, and to classify them into about twenty types. These tentative divisions will necessarily become more accurate as research proceeds, and the minor natural regions into which each major natural region should be divided will be definitely recognized, described and classified. Before this can be done, however, the study of geomorphology and of plant formations must be carried far beyond the present limits.

At the opposite end of the scale, that is, in the geographical study of localities, good work is beginning to be done. Dr. H. R. Mill, one of the pioneers of geography in this country and one of my most distinguished predecessors in this chair, has given us in his study of southwest Sussex an admirable example of a geographical monograph proper, which takes into account the whole of the geographical factors involved. He has employed quantitative methods as far as these could be applied, and in doing so has made a great step in advance. Quantitative determinations are at least as essential in geographical research as the consideration of the time factor.

The geomorphologist and the sociologist have also busied themselves with particular aspects of selected localities. Professor W. M. Davis, of Harvard, has published geomorphological monographs which are invaluable as models of what such work should be. In a number of cases he has passed beyond mere morphology and has called attention to the organic responses associated with each land form. Some of the monographs published under the supervision of the late Professor Ratzel, of Leipzig, bring out very clearly the relation between organic and inorganic distributions, and some of the monographs of the Le Play school incidentally do the same.

At present there is a double need. Research may take the form, in the first place, of collecting new information, or, in the second place, of working up the material which is continually being accumulated.

The first task—that of collecting new information—is no small one. In many cases it must be undertaken on a scale that can be financed only by governments. The ordnance and geological surveys of our own and other countries are examples of government departments carrying on this work. We need more of them. We need urgently a hydrographical department, which would cooperate with Dr. Mill's rainfall organization. It would be one of the tasks of this department to extend and coordinate the observations on river and lake discharge, which are so important from an economic or health point of view that various public bodies have had to make such investigations for the drainage areas which they control. Such research work as that done by Dr. Strahan for the Exe and Medway would be of the greatest value to such a department, which ought to prepare a map showing all existing water rights, public and private.

We shall see how serious the absence of such a department is if we consider how our water supply is limited, and how much of it is not used to the best advantage. We must know its average quantity and the extreme variations of supply. We must also

know what water is already assigned to the uses of persons and corporations, and what water is still available. We shall have to differentiate between water for the personal use of man and animals, and water for industrial purposes. The actualities and the potentialities can be ascertained and should be recorded and mapped.

In the second direction of research—that of treating from the geographical standpoint the data accumulated, whether by government departments or by private initiative—work has as yet hardly been begun.

The topographical work of the ordnance survey is the basis of all geographical work in our country. The survey has issued many excellent maps, none more so than the recently published half-inch contoured and hill-shaded maps with colors "in layers." Its maps are not all above criticism; for instance, few can be obtained for the whole kingdom having precisely the same symbols. It has not undertaken some of the work that should have been done by a national cartographic service—for instance, the lake survey. Nor has it yet done what the geological survey has done—published descriptive accounts of the facts represented on each sheet of the map. From every point of view this is a great defect; but in making these criticisms we must not forget (*a*) that the treasury is not always willing to find the necessary money, and (*b*) that the ordnance survey was primarily made for military purposes, and that the latest map it has issued has been prepared for military reasons. It has been carried out by men who were soldiers first and topographers after, and did not necessarily possess geographical interests. The ideal geographical map, with its accompanying geographical memoir, can be produced only by those who have had a geographical training. Dr. Mill, in the monograph already referred to, has shown us how to prepare systematized descriptions of the one-inch map sheets issued by the ordnance survey.

At Oxford we are continuing Dr. Mill's work. We require our diploma students to select some district shown on a sheet of this map for detailed study by means of map measurements, an examination of statistics and literature which throw light on the geographical conditions, and, above all, by field work in the selected district. Every year we are accumulating more of these district monographs, which ought, in their turn, to be used for compiling regional monographs dealing with the larger natural areas. In recent years excellent examples of such regional monographs have come from France and from Germany.

The preparation of such monographs would seem to fall within the province of the ordnance survey. If this is impossible, the American plan might be adopted. There the geological survey, which is also a topographical one, is glad to obtain the services of professors and lecturers who are willing to undertake work in the field during vacations. It should not be difficult to arrange similar cooperation between the universities and the ordnance survey in this country. At present the schools of geography at Oxford and at the London School of Economics are the only university departments which have paid attention to the preparation of such monographs, but other universities will probably fall into line. Both the universities and the ordnance survey would gain by such cooperation. The chief obstacle is the expense of publication. This might reasonably be made a charge on the ordnance survey, on condition that each monograph published were approved by a small com-

mittee on which both the universities and the ordnance survey were represented.

The information which many other government departments are accumulating would also become much more valuable if it were discussed geographically. Much excellent geographical work is done by the admiralty and the war office. The meteorological office collects statistics of the weather conditions from a limited number of stations; but its work is supplemented by private societies which are not well enough off to discuss the observations they publish with the detail which these observations deserve. The board of agriculture and fisheries has detailed statistical information as to crops and live stock for the geographer to work up. From the board of trade he would obtain industrial and commercial data, and from the local government board vital and other demographic statistics. At present most of the information of these departments is only published in statistical tables.

Statistics are all very well, but they are usually published in a tabular form, which is the least intelligible of all. Statistics should be mapped and not merely be set out in columns of figures. Many dull blue books would be more interesting and more widely used if their facts were properly mapped. I say *properly* mapped because most examples of so-called statistical maps are merely crude diagrams and are often actually misleading. It requires a knowledge of geography in addition to an understanding of statistical methods to prepare intelligible statistical maps. If Mr. Bosse's maps of population of England and Wales in Bartholomew's survey atlas are compared with ordinary ones the difference between a geographical map and a cartographic diagram will be easily appreciated.

The coming census, and to a certain extent the census of production, and probably the new land valuation, will give more valuable raw material for geographical treatment. If these are published merely in tabular form they will not be studied by any but a few experts. Give a geographer with a proper staff the task of mapping them in a truly geographical way and they will be eagerly examined even by the man in the street, who can not fail to learn from them. The presentation of the true state of the country in a clear, graphic and intelligible form is a patriotic piece of work which the government should undertake. It would add relatively little to the cost of the census and it would infinitely increase its value.

The double lack—the lacunæ in the information and the absence of adequate geographical treatment of such material as there is—makes the task of studying the huge natural divisions which we call continents a very difficult and unsatisfactory one. For several years in Oxford we have been trying to gather together the material available for the study of the continents and to make as accurate maps as is possible for geographical purposes. We have adopted uniform scales and methods, and by using equal area projections we have obtained comparative graphic representations of the facts. We hope before the end of the year to issue maps of physical features, vegetation and rainfall of each continent and other maps for the world. These are being measured, and I hope will yield more reliable quantitative information about the world and its continents than we possess at present.

With such quantitative information and with a fuller analysis of the major natural regions it ought to be possible to go a step further and to attempt to map the economic value of different regions at the present day. Such maps would necessarily be only approximations at first. Out of

them might grow other maps prophetic of economic possibilities. Prophecy in the scientific sense is an important outcome of geographical as well as of other scientific research. The test of geographical laws as of others is the pragmatic one. Prophecy is commonly but unduly derided. Mendelyeff's period law involved prophecies which have been splendidly verified. We no longer sneer at the weather prophet. Efficient action is based on knowledge of cause and consequence, and proves that a true forecast of the various factors has been made. Is it too much to look forward to the time when the geographical prospector, the geographer who can estimate potential geographical values, will be as common as and more reliable than the mining prospector?

The day will undoubtedly come when every government will have its geographical-statistical department dealing with its own and other countries—an information bureau for the administration corresponding to the department of special inquiries at the board of education. There is no geographical staff to deal geographically with economic matters or with administrative matters. Yet the recognition of and proper estimation of the geographical factor is going to be more and more important as the uttermost ends of the earth are bound together by visible steel lines and steel vessels or invisible impulses which require no artificial path or vessel as their vehicle.

The development of geographical research along these lines in our own country could give us an intelligence department of the kind, which is much needed. If this were also done by other states within the empire, an imperial intelligence department would gradually develop. Thinking in continents, to borrow an apt phrase from one of my predecessors, might then become part of the necessary equipment of a statesman instead of merely an after-dinner aspiration. The country which first gives this training to its statesmen will have an immeasurable advantage in the struggle for existence.

Our universities will naturally be the places where the men fit to constitute such an intelligence department will be trained. It is encouraging, therefore, to see that they are taking up a new attitude towards geography, and that the civil service commissioners, by making it a subject for the highest civil service examinations, are doing much to strengthen the hands of the universities. When the British Association last met in Sheffield geography was the most despised of school subjects, and it was quite unknown in the universities. It owed its first recognition as a subject of university status to the generous financial support of the Royal Geographical Society and the brilliant teaching of Mr. Mackinder at Oxford. Ten years ago schools of geography were struggling into existence at Oxford and Cambridge, under the auspices of the Royal Geographical Society. A single decade has seen the example of Oxford and Cambridge followed by nearly every university in Great Britain, the University of Sheffield among them. In Dr. Rudmore Brown it has secured a traveler and explorer of exceptionally wide experience, who will doubtless build up a department of geography worthy of this great industrial capital. The difficulty, however, in all universities is to find the funds necessary for the endowment, equipment and working expenses of a geographical department of the first rank. Such a department requires expensive instruments and apparatus, and, since the geographer has to take the whole world as his subject, it must spend largely on collecting, storing and utilizing raw material of the kind I have

spoken of. Moreover, a professor of geography should have seen much of the world before he is appointed, and it ought to be an important part of his professional duties to travel frequently and far. I have never been able to settle to my own satisfaction the maximum income which a department of geography might usefully spend, but I have had considerable experience of working a department with an income not very far above the minimum. Till this year the Oxford School of Geography has been obliged to content itself with three rooms and to make these suffice not merely for lecture-rooms and laboratories, but also for housing its large and valuable collection of maps and other materials. This collection is far beyond anything which any other university in this country possesses, but it shrinks into insignificance beside that of a rich and adequately supported geographical department like that of the University of Berlin. This fortunate department has an income of about 6,000*l.* a year and an institute built specially for its requirements at a cost of over 150,000*l.*, excluding the site. In Oxford we are only too grateful that the generosity of Mr. Bailey, of Johannesburg, has enabled the school of geography to add to its accommodation by renting for five years a private house, in which there will temporarily be room for our students and for our collections, but where we can never hope to do what we might if we had a building specially designed for geographical teaching and research. Again, Lord Brassey and Mr. Douglas Freshfield, a former president of this section, have each generously offered 500*l.* towards the endowment of a professorship if other support is forthcoming. All this is matter for congratulation, but I need hardly point out that a professor with only a precarious income for his department is a person in a far from enviable position. There is at present no permanent working income guaranteed to any geographical department in the country, and so long as this is the case the work of all these departments will be hampered and the training of a succession of competent men retarded. I do not think that I can conclude this brief address better than by appealing to those princes of industry who have made this great city what it is to provide for the geographical department of their university on a scale which shall make it at once a model and a stimulus to every other university in the country and to all benefactors of universities.

A. J. Herbertson.

University of Oxford

THE ASSOCIATION OF AMERICAN UNIVERSITIES

We learn from a report in the New York *Evening Post* that the Association of American Universities met at the University of Virginia last week. Three papers were presented by delegates. The first, by President Bryan, of Indiana University, was on "Allowing Credit for Professional Work to Count toward the Degree of Bachelor of Arts."

President Bryan is in favor of allowing students to complete requirements for this degree in a standard college of arts and sciences, and in a professional school, in seven years where the professional course requires four years, and in six years where the professional course requires three; also of granting two degrees when the work for them has been done simultaneously, but separately. Emphasis was laid upon the statement that there should be no discrimination against colleges connected with universities.

The second paper, by Professor Calvin Thomas, "The Degree of Master of Arts," defended the two propositions: that work for this degree should require intensive work in one study for at least one year, and that the candidate should have a bachelor's degree

from some approved college. On the second day of the meeting President Van Hise, of the University of Wisconsin, discussed "The Appointment and Tenure of University Professors."

At the annual election, the University of Virginia was chosen president of the association, the University of Illinois vice-president and the University of Missouri a member of the executive committee. Harvard remains secretary and Columbia was reelected a member of the executive committee. Chicago was chosen as the next place of meeting. The work closed with a conference of deans, the most notable matter of discussion being the entrance requirements for graduate schools.

The delegates were: Dean Barrows, of California; Professor Bolling, of the Catholic University of America; President Judson and Dean Salisbury, of Chicago; Professor Sanford, of Clark; Professors Carpenter and Thomas, of Columbia; Dean Merritt and Professor Thilly, of Cornell; President Lowell, Dean Haskins and Secretary Little, of Harvard; Director Davenport, of Illinois; President Bryan and Dean Hoffman, of Indiana, Deans Wilcox and Seashore, of Iowa; Professor Ames, of Johns Hopkins; Deans Blackmer, of Kansas; Reed, of Michigan, and Downey, of Minnesota; President Hill, of Missouri; Deans Sherman, of Nebraska, and Ames and Fisher, of Pennsylvania; Dean West and Professor Conklin, of Princeton; President Jordan, of Leland Stanford Junior; President Van Hise, of Wisconsin; Dean Jones and Director Chittenden, of Yale, and President Alderman, Deans Page, Thornton, Lile, Dabney and Whitehead and Professors Tuttle, Kent, Wilson, Fitz-Hugh, Payne, Kastle and Faulkner, of Virginia.

THE SALARIES OF PROFESSORS OF YALE UNIVERSITY

Attention has already been called in Science to increases in the salaries of professors at Yale University. The official announcement of the action taken by the corporation is as follows:

Owing to the generosity of the alumni, a sum of about $50,000 contributed by the Alumni Fund Association, was available for this purpose (professorial salary increases). Of this $10,000 was set apart last spring for salary increases. Of the remainder, one third is now appropriated to increase the salaries of assistant professors and two thirds to increase the salaries of full professors. The new salary scale put into force continues instructors at from $1,000 to $1,600, increases assistant professors on the first term of appointment from $1,800 to $2,000, sets $2,500 as the normal scale for the second appointment of the assistant professors' grade and reduces the period of the second appointment from five years to three years, making $3,000 the normal salary for an assistant professor after the expiration of his second term.

All of the assistant professors of the first grade in the two undergraduate departments had their salaries raised to the new scale for the coming year.

It is believed that the new salary scale avoids the danger of an absolutely fixed scale on the one hand and of haphazard determination of individual salaries on the other. In the case of assistant professors the normal salary plan of the past is continued and strengthened, although the corporation reserves the right to withhold salary increases where the work is unsatisfactory.

In the case of professors, normal grades of $4,000, $4,500, and $5,000 are adopted—with length of service, university responsibility and individual distinction as a scholar or teacher forming the criteria on which the president and dean of a department base their recommendations for advance to the corporation. On this plan twelve professors in the university were placed on the maximum salary of $5,000 and a slightly larger number on the $4,500 basis.

SCIENTIFIC NOTES AND NEWS

Dr. Edgar F. Smith, for twenty-two years professor of chemistry in the University of Pennsylvania and for twelve years vice-provost, has been elected provost in succession to Dr. C. C. Harrison.

We have not been able to obtain an authorized statement of the degrees conferred at the celebration of the centenary of the University of Berlin, but it appears that three honorary degrees were conferred on American men of science—the degree of doctor of philosophy on

Dr. George E. Hale, director of the Mount Wilson Solar Observatory, and on Dr. Bailey Willis, of the U. S. Geological Survey, and the degree of doctor of medicine and surgery on Dr. Theodore W. Richards, professor of chemistry in Harvard University.

It is reported that the Nobel prize for physics will be awarded to Professor J. D. van der Waals, of Amsterdam, for his work on gases and liquids.

Professor Edward B. Poulton, Professor T. H. Morgan and Dr. L. O. Howard have been elected correspondents of the Academy of Natural Sciences of Philadelphia.

The Royal Society of Edinburgh has elected honorary fellows as follows: *British*—Professor J. G. Frazer, Sir Joseph Larmor, F.R.S., Dr. Alfred Russel Wallace, O.M., F.R.S.; *foreign*—Professor Hugo de Vries, Amsterdam; Mr. F. A. Forel, Morges; Professor Karl F. von Goebel, Munich; Professor J. C. Kepteyn, Gröningen; Professor Elie Metchnikoff, Paris; Professor A. A. Michelson, F.R.S., Chicago; Professor W. Ostwald, Leipzig; Professor F. W. Putnam, Harvard University, and Professor A. F. L. Weismann, Freiburg.

At the meeting of the trustees of the Carnegie Foundation for the Advancement of Teaching, held in New York City last week, President A. Lawrence Lowell, of Harvard University; President W. L. Bryan, of Indiana University, and President J. M. Taylor, of Vassar College, were elected trustees to fill vacancies caused by the retirement of Dr. Woodrow Wilson, formerly president of Princeton University; Dr. C. C. Harrison, retiring provost of the University of Pennsylvania, and Dr. L. Clarke Seelye, formerly president of Smith College. No other information in regard to the action of the trustees appears to have been made public.

The Royal Society's medals will this year be awarded as follows: The Copley medal to Sir Francis Galton, F.R.S., for his researches on heredity; the Rumford medal to Professor Heinrich Rubens, for his researches on radiation, especially of long wave-length; a Royal medal to Professor Frederick O. Bower, F.R.S., for his treatise on the origin of a land flora; a Royal medal to Professor John Joly, F.R.S., for his researches in physics and geology; the Davy medal to Professor Theodore W. Richards, for his researches on the determination of atomic weights; the Darwin medal to Mr. Roland Trimen, F.R.S., for his South African bionomic researches, in large part undertaken as the outcome of correspondence with Charles Darwin; the Sylvester medal to Dr. Henry F. Baker, F.R.S., for his researches in the theory of Abelian functions and for his edition of Sylvester's "Collected Works"; the Hughes medal to Professor John A. Fleming, F.R.S., for his researches in electricity and electrical measurements.

Officers of the Royal Society have been nominated by the council as follows: *President,* Sir Archibald Geikie, K.C.B.; *treasurer,* Mr. Alfred Bray Kempe; *secretaries,* Sir Joseph Larmor and Dr. John Rose Bradford; *foreign secretary,* Sir William Crookes; *other members of the council,* Mr. L. Fletcher, Dr. W. H. Gaskell, Sir David Gill, K.C.B., Dr. E. H. Griffiths, Professor W. M. Hicks, Professor F. S. Kipping, Major P. A. MacMahon, Mr. H. R. A. Mallock, Dr. C. J. Martin, the Duke of Northumberland, K.G., Professor W. J. Pope, Professor J. H. Poynting, Professor E. Rutherford, Mr. A. E. Shipley, Mr. M. R. Oldfield Thomas and Mr. Harold W. T. Wager.

Professor Joseph Baker Davis, who has been a member of the faculty of the engineering department of the University of Michigan since 1872, resigned at the October meeting of the regents. In recognition of his services to the university he has been made professor emeritus.

The studentship on the foundation of the late Professor Tyndall for scientific research on subjects tending to improve the conditions to which miners are subject has been awarded for the ensuing year to Dr. T. L. Llewellyn, of Bargoed, Wales, for research regarding the cause and cure of the disease in miners known as nystagmus.

Mr. Theodore Roosevelt has taken advantage of a recent stay in Washington to inspect the collections made by the Smithsonian African Expedition during 1909–10.

Mr. A. S. Hitchcock, systematic agrostologist, U. S. Department of Agriculture, has returned from a four months' trip to Mexico. He brought back a collection of grasses, consisting of 2,703 numbers, about 20,000 specimens, made in forty localities from nearly all the states north of the Isthmus of Tehuantepec. He was accompanied by his son, Frank Hitchcock, as assistant.

Professor Frederick Starr, of the University of Chicago, will leave the United States on December 22 for Korea, where he will make a study of the inhabitants. Mr. Manuel Gonzales, his companion on previous trips of this kind, will accompany him.

Mr. Priestly, who accompanied Sir Ernest Shackleton, as geologist, on his Antarctic expedition, is going with Captain Scott in the place of Mr. Thompson, who is ill.

A large shipment of pure rare earths, worth several thousand dollars, has been received by Professor Victor Lenher, of the chemistry department of the University of Wisconsin. Several graduate students will also make use of the material in their investigations.

For the past three years advanced lecture and seminary courses in physics, chemistry and mathematics have been maintained at the Bureau of Standards by the members of the staff, primarily in the interests of those of the younger men who have not yet received the doctorate. For the current year such courses are being given by Drs. E. B. Rosa and A. S. McDaniel, of the bureau, and Drs. J. A. Anderson and A. H. Pfund, of Johns Hopkins University. Over thirty men are enrolled in the courses.

Dr. L. H. Bolley, botanist at the North Dakota Experiment Station, addressed the department of plant morphology of the New York State College of Agriculture on Friday evening, November 11. The subject of his remarks was, "The Relation of Fungous Diseases to Soil Sanitation and Crop Rotation."

The annual Huxley memorial lecture of the Royal Anthropological Institute was given on November 22 by Professor W. Boyd Dawkins, F.R.S., whose subject was "The Arrival of Man in Britain in the Pleistocene Age."

Dr. Henry Wurtz, formerly chemical examiner in the U. S. Patent Office, has died at his home in Brooklyn, in his eighty-third year.

Mr. Theodore Cooke, for many years principal of the Poona College of Science, has died at seventy-four years of age.

Dr. D. J. B. Gernez, member of the Paris Academy of Sciences, associated with Pasteur in his researches, has died at the age of seventy-six years.

As the Chicago Section of the American Mathematical Society will meet at Minneapolis in affiliation with the American Association, all mathematical papers which are to be presented at this meeting should be mailed to Professor H. E. Slaught, University of Chicago. The papers on astronomy should be mailed to the secretary of Section A. The reading of the latter will begin on Wednesday morning immediately after the organization of the section. The "general-interest session" of Section A will be held on Wednesday afternoon. At this session the retiring vice-president, Professor E. W. Brown, will read his address entitled "The relations between Jupiter and the Asteroids." Several other papers of general mathematical and astronomical interest will be read at this session. On Friday afternoon there will be a joint session of Sections A and D and the Chicago Section of the American Mathematical Society to hear the report of the committee on the teaching of mathematics to engineering students, which was appointed at a similar joint meeting at Chicago in 1907. Dr. G. A. Miller, of the University of Illinois, is secretary of the section.

The American Phytopathological Society will hold its annual meeting in Minneapolis, Minnesota, in affiliation with the American Association for the Advancement of Science, December 28–30. The society will hold joint sessions with the Botanical Society of Amer-

ica and Section G of the American Association. Dr. F. L. Stevens, of West Raleigh, North Carolina, is the president, and Dr. C. L. Shear, of Washington, D. C., secretary of the society.

The annual meeting of the Home Economics Association under the presidency of Mrs. Ellen H. Richards, of the Massachusetts Institute of Technology, will be held at St. Louis at the time of the meeting of the American Economic Association and the American Sociological Association, December 27–30. Interest will center in the joint discussions by both associations on the teaching of economics in the high schools. The program of the Home Economic Association meeting may be secured by addressing the secretary, Benjamin R. Andrews, Teachers College, Columbia University.

The American Museum of Natural History receives $25,000 by the will of the late Charles E. Tilford, of New York City.

According to a cablegram to the daily papers, a ceremony in celebration of the completion of the Vatican observatory under the direction of Father Hagen was held in the papal apartment on November 17. A speech was made by Cardinal Maffi, president of the observatory, to which the pope replied, highly commending the work of Father Hagen.

The Field Museum of Natural History at Chicago announces its intention of making a study of the fresh-water, brackish and salt-water shore fish of the Canal Zone during the coming dry season. The study will be made before the completion of the canal, in order that the fish may be taken before they have had a chance to cross the divide and undergo the changes incident to a new environment.

Last June Mr. Jake Gimbel, a merchant of Vincennes, Indiana, made possible the Gimbel expedition to British Guiana for the study of the fish of the family Gymnotidæ. During the summer this expedition was jointly equipped by Mr. Gimbel and Indiana University and on August 24 Mr. Max M. Ellis, A.M. (Indiana), teaching fellow in zoology, with Mr. Wm. Tucker, A.M. (Indiana), as assistant, sailed via Ruebec Line for Georgetown. General collections were made in the Demerara and Essequibo rivers as well as Hubabu and Palmachusia creeks, supplementing those made by Dr. C. H. Eigenmann in 1908. A considerable amount of time was given to experimental work in and about Georgetown. Messrs. Ellis and Tucker returned to the United States on October 29.

The department of plant pathology of the New York State College of Agriculture announces the establishment of three more industrial fellowships, as follows: The Ten Broeck fellowship, established by Wessel Ten Broeck, for the investigation of the effect of cement dust on the setting of fruit, injury to foliage, etc. The work on this fellowship, during the growing season, is conducted in a field laboratory near a large cement plant at Hudson, N. Y. Mr. P. J. Anderson, holder of the fellowship, is a 1910 graduate of Wabash College. The Byron fellowship, established by the Byron Fruit Growers' Association, of South Byron, N. Y., for the investigation of the diseases of fruit trees. Special attention will be given to the New York apple tree canker. The holder of this fellowship is Mr. Lex R. Hesler. He completes his work at Wabash College January 1, 1911. The Bethany-Batavia fellowship, established by the Bethany-Batavia Fruit Growers' Association near Batavia, N. Y., for the investigation of the diseases and insect pests of orchard crops, especially the diseases of apples. Particular attention will be given to the use of sulphur fungicides for the control of these diseases and pests. This fellowship as well as the Byron fellowship is established jointly in the departments of plant pathology and entomology. It provides for two fellows, one in entomology and the other in plant pathology. The work on this fellowship will begin in the spring of 1911.

UNIVERSITY AND EDUCATIONAL NEWS

It is announced that Mr. Andrew Carnegie has given a further sum of $1,500,000 for the construction of buildings of the Carnegie Technical Schools at Pittsburgh.

BY the will of Professor A. Marshall Elliott, the Johns Hopkins University receives his library, and the sum of $2,000 for the establishment of a scholarship for graduate students in the department of Romance languages.

THE twenty-fourth annual convention of the Association of Colleges and Preparatory Schools of the Middle States and Maryland, will be held at Lehigh University on November 26 and 27. The three sessions of the convention will be devoted to the discussion of mathematics, science and English respectively. President Drinker, of Lehigh, will give the address of welcome. The address of Dr. J. M. Greene, of the New Jersey State Normal School, president of the association, will be on "Educational Economics."

WE learn from *Nature* that the Duke of Connaught on November 5 laid the foundation-stone of the new university hall of the Cape University. The council of the university presented an address, in which the hope was expressed that the union now accomplished in South Africa would lead to the conversion of the present Cape University into a teaching university for the whole of South Africa, by incorporating existing institutions of higher education as constituent colleges, and by creating chairs for those subjects for which no single college could provide. In replying, the Duke of Connaught said he trusted that the funds necessary to convert the Cape University into a great teaching university would be forthcoming. At a university luncheon, held on the same day, Mr. Malan, minister of education, announced that Mr. Otto Beit had agreed to divert the sum of £200,000, bequeathed by the late Mr. Alfred Beit for the foundation of a university at Johannesburg, to the creation of a great teaching university at Groote Schuur, the estate of the late Mr. Cecil Rhodes outside Cape Town. It was also announced that Sir Julius Wernher has promised to make up the amount to a total of £500,000.

AT the University of Virginia the following promotions have been made: J. L. Newcomb to be professor of engineering, Edgar Graham to be adjunct professor of chemistry; David Vance Guthrie to be adjunct professor of physics. W. S. Rodman, of the Massachusetts Institute of Technology, has been appointed adjunct professor of electrical engineering.

DISCUSSION AND CORRESPONDENCE

THE REFORM OF THE CALENDAR

TO THE EDITOR OF SCIENCE: The several suggestions for the simplification of the current calendar made in your columns by Reininghaus (July 29), Slocum, embracing those of Cotsworth (September 2), Patterson (October 14) and Dabney (October 21) awaken the hope that a calendar can be contrived which will be much superior to the present one and which at the same time will not encounter so much prejudice and human inertia as to be fatal to its adoption at an early date. It is, however, of the first importance that the new calendar be so well matured before its adoption is seriously urged that it will not itself need to be laid aside for something better by the time it has fairly come into use. To this end suggestions from various points of view followed by a period of deliberate study and tentative combination may well be regarded as indispensable to the best ultimate results. As a possible contribution to this preliminary work, I venture to suggest a calendar that embodies many of the excellent suggestions already made, but instead of introducing a 13th month, makes use of only 12 months of 4 weeks (28 days) each, bunching these into four groups and placing the remaining four weeks between these groups so as to set out the four seasonal quarters of the year distinctly. The purpose is to facilitate the use of the *quarters* of the year as convenient time divisions of an order intermediate between the month and the year. The quarters of the year already have a large place in the accountings of the industrial and financial world and are likely to grow into very important time divisions.

The integers of the proposed scheme are these:

(*a*) *Quarters:* Corresponding measurably to the *four seasons.*

(*b*) *Months:* 12 of 28 days (4 weeks) each, assembled in groups of three terminated by a single closing week with a special designation. All months to begin on Monday, as suggested by Patterson.

(*c*) *Weeks:* 52 of 7 days each, all beginning with Monday. Forty-eight of the weeks, in groups of 4 each, constitute the 12 months. The remaining 4 weeks of the 52, viz., the 13th, the 26th, the 39th and the 52d, to be attached severally at the end of the four three-month groups to make up four symmetrical quarters of 13 weeks each. These terminal weeks might be designated as closing or quarter-end weeks; but each is to have its own special name, the 13th to be Easter week, the 26th Julian week, the 39th Gregorian week and the 52d Christmas week. In large measure these might concentrate into themselves the holidays, short vacations, book-closing periods, etc.; and so come to have other special designations suited to the various vocations.

(*d*) *Odd Days:* The odd day of the usual year, the 365th day, to be New Year's Day, and to be *dies non* so far as the week and the month are concerned, as proposed by Patterson, but to be grouped with the preceding quarter as the end-day of the old year and as the start-day of the new year. The adjustment for the odd one-quarter day to follow the Julian method and to be made by a Leap Day following New Year's Day every fourth year, and to be a *dies non* also so far as week and month are concerned, as also proposed by Patterson, but to be grouped with the preceding quarter.

The necessary correction of the Julian reckoning to be made by the Gregorian method as now, by means of the periodic omission of the Leap Day.

Some further details of the scheme, particularly the places and names of the transition or quarter-end weeks, will appear in the following table which throws the scheme into form:

FIRST QUARTER

(Winter season, northern hemisphere)
(Summer season, southern hemisphere)

January—4 weeks, 28 days.
February—4 weeks, 28 days.
March—4 weeks, 28 days.
Close Week—Easter week.

SECOND QUARTER

(Spring season, northern hemisphere)
(Fall season, southern hemisphere)

April—4 weeks, 28 days.
May—4 weeks, 28 days.
June—4 weeks, 28 days.
Close Week—Julian week.

THIRD QUARTER

(Summer season, northern hemisphere)
(Winter season, southern hemisphere)

July—4 weeks, 28 days.
August—4 weeks, 28 days.
September—4 weeks, 28 days.
Close Week—Gregorian week.

FOURTH QUARTER

(Fall season, northern hemisphere)
(Spring season, southern hemisphere)

October—4 weeks, 28 days.
November—4 weeks, 28 days.
December—4 weeks, 28 days.
Close Week—Christmas week and odd days.

The special feature of the scheme is the symmetrical assembling of twelve months of strictly uniform composition into four seasonal groups with a close week each. Each group therefore consists of 13 weeks and together they embrace the 52 weeks of the year. The odd days are placed so as to emphasize the Christmas holidays that mark the close of one year and the beginning of the next. By thus using the odd days to emphasize the transitions between the years and the odd weeks above those that make up the twelve uniform months to mark the transitions between the quarters, almost perfect symmetry is secured, and the close weeks of the quarters should lend their good offices to secure uniformity of practise in the periodic work of the world of affairs, of society and of education.

The feature that is perhaps most debatable in this scheme is the shifting of March, June, September and December forward in the seasonal scheme. This avoids dividing the winter season (summer season of southern hemisphere) between two years, which is our present method in the grouping of the months

into seasons, but which is usually ignored in commercial and educational practise. The quarters as grouped in the scheme proposed are those that are most recognized in the current time divisions of the business, social and educational worlds. There are indeed climatic reasons for our seasonal grouping in disregard of the year division, but, after all, March is only slightly less wintry than December, and June scarcely more summery than September. A strict seasonal adjustment is embarrassed by the lag of the climatic effects behind their astronomic causes and by the opposite phases which the seasons assume in the northern and southern hemispheres. In the tropics the influence of conditions other than the sun's position on the nature of the seasons adds to the difficulty. To this is added also the lack of strict adjustment of either the present or the proposed calendar to the astronomical divisions initiated by the equinoxes and solstices. Even if a strict adjustment of the calendar to these were made, the climatic effects would lag behind the astronomical divisions in a vague and fluctuating way. Under the proposed scheme, each seasonal quarter would start about ten days after the astronomic event that may be said to initiate it. This may be construed as some recognition of the lag of climatic effect, though it is merely accepting current usage in starting the new year.

With the shifting of the months as proposed, and accepting the ten-day delay as a compromise lag, each quarter would mark a climatic movement of a single kind, a phase of increase of insolation or a phase of decrease of insolation; the winter, a movement from the lowest insolation in the northern hemisphere to medium insolation (the opposite, of course, in the southern hemisphere); the spring, a movement from medium insolation to the highest insolation; the summer and the fall, the corresponding phases of decrease. While as systematists and as scientists we might prefer a shift from present usage to the exact dates of the solstices and equinoxes, it would probably be asking too much of the inertia of mankind to change the calendar so as to effect this. Besides, these astronomical divisions are not strictly equal, and that would give us trouble.

In the matter of holidays, the scheme seems to lend itself fairly well to current practise and is perhaps well suited to mold future practise as well. The 28th day of December would always fall on Sunday and be the immediate fore-runner of Christmas. Christmas itself would always fall on the Monday of Christmas week. Our greatest holiday would thus have a distinctive place of its own at the head of its special week, instead of falling in the midst of a month and on a constantly shifting day of the week. The winter holiday season would be closed usually by New Year's Day, but on every fourth year by Leap Day, following New Year's Day. The Christmas holidays would thus be lengthened to nine days or to ten days.

Easter week would always begin on Monday, the 85th day of the year, and the days of the week might have the special designations, Easter Tuesday, Easter Wednesday, and so on, ending with Easter Sunday, which would appropriately be followed by the spring season.

The Julian week would embrace our national holiday, which would always be Julian Thursday. The Julian week might well come to embrace the observances that mark the end of the educational year.

The Gregorian week would fall at a time well suited to the harvest festivals, the fairs, etc.

The close weeks between each of the three-month groups would form a natural time for closing books for the quarter, rounding up accounts, making out quarterly reports, holding official corporation meetings, declaring dividends, etc., in the world of affairs, and for vacations and rest intervals in the educational and professional worlds.

The authors of the rectifications that gave us our present calendar are recognized in the naming of the Julian and Gregorian weeks.

T. C. Chamberlin

ANTARCTICA AS A FORMER LAND CONNECTION BETWEEN THE SOUTHERN CONTINENTS

Lest my position with reference to this subject be misunderstood, I wish to state that my

letter entitled "Shackleton's Contribution to Biogeography" was intended as suggestive rather than positive. Such may be gathered from the opening paragraph. But perhaps some may conclude from it that I view Antarctica as practically the only land connection that has existed between the various continental masses, or between the eastern and western hemispheres; that I consider that faunal and floral elements have ever traveled northward, and have reached the northern continents through the southern from Antarctica as a center of dispersal. This is not the idea that I wish to convey. There have been northward and southward dispersals of types, and eastward and westward dispersals as well. There has certainly been land connection between Eurasia and North America in the Bering region in times past, and many types have passed over that bridge, some going one way and some the other. There are distinctively northern types that have spread southward into the continents of the southern hemisphere, but there are also southern types that have spread northward. There is evidence for believing that the southward dispersals are much more recent than the northward, taken as a whole.

The biogeographical rôle which I conceive Antarctica to have played is that of a center of distribution of the earlier forms of life; and later, perhaps, an exchange ground between the southern continents for old-type stocks, many or some of which traveled northward into the continents of the northern hemisphere. The main point after establishing the Antarctic contacts is to determine the length of their duration with the southern continents. It is quite possible that none of these contacts persisted into Tertiary time, and my suggestion that Africa may have been connected with the southern mass until the Miocene is perhaps wide of the mark. The distribution of certain forms which prompted the suggestion may have to be otherwise explained. It was during Paleozoic and Mesozoic times that I consider Antarctica to have been especially active as a center of dispersal and exchange. I should better have said in my former communication that during Mesozoic (Jurassic to Cretaceous) times the continent was probably not dissimilar to present-day South America and Asia in average elevation; and that its subsequent further uplifting, together with the enormous weight at times of its ice-sheet, which has repeatedly extended and decreased in accordance with changing conditions of altitude and temperature, caused its shelf lines to sink beneath the ocean. Its complete isolation may have been effected before the Tertiary. On the other hand, some of its contacts may have prevailed until the Oligocene. At all events there seems evidence enough to indicate that at one time it played an important part in the dispersal of old-type forms. Doubtless Antarctica is the most remotely ancient of the continents, as indicated by its greatest average elevation.

I am well aware that most European and North American students consider the great majority of distinctively Tertiary and later types to have originated in the northern hemisphere, and I may add that this is also my opinion. But it is well known that, at the beginning of the Tertiary, many types of the higher insects were already in existence, some of which have persisted with little change to the present day. This must necessarily have been the case, since we find their remains scattered through Eocene, Oligocene and Miocene deposits. While evolution has undoubtedly been most active in the development of new types in the northern hemisphere since early Tertiary time, and the prevailing trend of migration has consequently been to the southward, there seems much evidence to indicate that prior to that time the reverse was the case in large part at least.

I shall be glad to see an expression of views on this subject from paleontologists who have made especial studies of various phyla.

Charles H. T. Townsend

Piura, Peru,
September 27, 1910

AMERICAN EDUCATION

To the Editor of Science: The criticisms passed on American education by Mr. Gunn

in your recent issue would be engaging in their frankness, did they not suffer from the vice of banality. It is such a commonplace to start with the premise that this nation is through and through "commercial," and to deduce therefrom the conclusion that our colleges and universities are commercialized, from which, in turn, all the deficiencies of our educational practise are explained. This deductive method, which would now-a-days be dismissed as absurd in the natural sciences, is still the common approach to educational problems, and is precisely the method which must be gotten away from before educational reform can have a scientific basis. Another defect of the author's method is a loose use of such terms as "commercial." Now commerce is at once a gigantic business and a pursuit of gain. In the latter respect it does not differ, to be sure, from other economic activities, yet its name, when used as a tool of deprecation, seems to contain a reference to sordid profits. If, however, the author, and others who talk in the same vein, wish to convey this meaning when they speak of the administration of American colleges as commercialized, they are certainly far afield; for no evidence, as far as I know, has been presented tending to show that presidents and trustees so administer as to make profits for themselves. Probably the author does not mean to include this particular implication of "commercial" when he speaks of college administration, though he does distinctly include it when he turns to apply the term to the student and his aims. As applied to the administration, "commercialized" probably means "desirous of doing big business"; but certainly a more precise characterization of American university administration is necessary before its excellencies can be intelligently strengthened or its vices corrected. As applied to the teaching force of our universities, the author's stock adjectives apparently mean neither that the professor is intent above all things on gain, nor that he is enamored of the ideal of great enterprises, but rather that the atmosphere of American life makes it impossible for him, or for any one, to enter upon any but commercial pursuits with entire seriousness and enthusiasm. Hence the professor, if naturally energetic, becomes a pedant, or, otherwise, a dilettante; in neither case can he be an inspiring teacher, or rise to true scholarship; in consequence of which the nation's achievements in pure science "have been insignificant." A third defect of the author's method appears in these superlatives and absolute statements, when comparative measures can alone represent the truth or afford a basis and incentive for advance. What we need is the facts, inductively determined, accurately formulated, and if possible put into such shape that quantitative comparisons may be possible between our own conditions and those in more advanced countries, and between our condition now and hitherto as well as hereafter. I have no doubt, however, that such a suggestion will appear to the author as simply one more illustration of that commercial tendency which forms the chief weakness of American education and scholarship—"a disposition to deal with facts and to neglect principles."

R. S. WOODWORTH

COLUMBIA UNIVERSITY,
October 29, 1910

SCIENTIFIC BOOKS

The Laws of Heredity. By G. ARCHDALL REID, M.B., F.R.S.E. London, Methuen & Co. 1910.

Dr. Archdall Reid has already given us books and articles on heredity that are both interesting and instructive, and the present volume not only lives up to the standards set by its predecessors in these particulars, but surpasses them in the breadth of its scope, which is much greater than its title would seem to imply. For not only does the author give an exposition of the laws of heredity and abundantly criticize them, but he discusses at length their bearings, as he sees them, on such sociological questions as eugenics, intemperance, insanity and education, on such psychological problems as the relation of mind to

body, instinct, reason and memory, and withal takes occasion to present disquisitions on the method of science, on the relative values of induction and deduction, on idealism and common sense, and one chapter bears the Teufelsdröckhian heading "Necessary Truth."

The book is an output of the study and the author glories in that it is. Laboratory and statistical methods are in his opinion practically superfluous in the study of heredity; simple observation of patent facts and deduction alone are essential.

> Not seldom in biometric inquiries . . . several scores or hundreds of observers and thinkers are employed for years in ascertaining, with a much less degree of certainty, that which a single thinker may deduce in two minutes from known and admitted truths.
>
> If the reader will think over the evidence on which I shall draw for the purpose of the present volume, I believe he will conclude that, if any of it bears a doubtful aspect to his mind, it is that large mass which has been furnished by laboratory inquiry.

These quotations reveal Dr. Reid's attitude toward two popular methods of modern biological inquiry and, at the same time, they reveal his limitations as a critic. And an additional imperfection in his treatment of his subject is the failure to take sufficiently into account the known complexity of cell structure and the bearings of this on inheritance. For him the cell is the ultimate unit and even in these days of atom-splitting deductions we find in his philosophy no consideration of any lower structural or physiological units, no suggestion of the important bearings which our knowledge of the architecture of germ plasm may have on the subjects he discusses, and this because, in his opinion, the discussion of such matters would be merely "valueless guessing—valueless because incapable of being tested"! Why they can not be tested like any other theories or facts it is difficult to understand.

But in spite of these limitations, indeed, to some extent because of them, Dr. Reid has given us a book full of suggestive ideas. Indeed, so full is it of suggestions that it will be impossible here to do more than give a brief outline of some of his conclusions. What may be regarded as the basis of his argument is the idea that "Evolution is only another name for adaptation and in the last analysis all adaptation results from the natural selection of favourable variations." This is the Neo-Darwinian creed, but Dr. Reid adds to it the idea that it is not so much the evolution of definite characters that is the office of natural selection as it is the provision of possibilities for variation and the regulation of their magnitude. Growth is the result of stimuli, such as nutrition, injury and use, and natural selection has brought about such responses to these stimuli as place the organism in adaptation to its surroundings. Thus it is not a large muscle nor an elongated neck that is inherited, but the possibility of developing these peculiarities under the influence of the stimulus of nutrition or use. Inheritance of acquired characters does not occur, it is not the character that is inherited and no character is any more acquired or innate than another. A serious fallacy in the Lamarckian position, according to Dr. Reid, is that it demands that a structure enlarged under the stimulus of use, for example, should give in another generation a similar response to the altogether different stimulus of nutrition. For him the Lamarckian position is foolishness; "it is dead as an accepted interpretation of the facts."

To the question that underlies the development of possibilities of variation, the cause of variation, Dr. Reid gives no satisfactory answer or theory. What is inherited is the germ plasm, and variations in this may be produced either "spontaneously" or by the action of the environment, this meaning apparently the external environment. The latter cause is of little moment, since it can act only injuriously and can therefore have been of but rare occurrence in individuals that survive and reproduce. It is on "spontaneous" variations alone that natural selection acts and this spontaneous variability has itself been evolved under the action of natural selection. But how these "spontaneous" variations occur is not even hinted at and on this point we are

left where Darwin left us, without any clue to the manner in which natural selection is supplied with the material on which it may act.

The biogenetic law reappears in all its pristine vigor, indeed with added vigor, for it is held to be inconceivable that the phylogenetic recapitulation should not occur if it be granted that species arose by evolution and that the offspring recapitulates the parental development. Both progressive and retrogressive modifications of the recapitulation occur, and of these the retrogressive ones tend to be the greater either in frequency or magnitude or in both. Retrogressive modifications are omissions from the complete recapitulation and are therefore identical with reversions. The omission may, however, be merely apparent in some cases; an ancestral trait may appear to have been dropped when in reality it is merely latent, and hence the reappearance of a dormant ancestral trait is not a reversion. Regression in the Galtonian sense is merely the first stage of retrogression.

The object of sexual reproduction is not the production of variations, since these occur with parthenogenesis; its function is the blending of parental characters. Certain characters, however, are alternative and among these are sexual characters, using the term sexual in the widest sense. Thus every individual possesses three sets of characters, one set common to both sexes, that is to say, patent in both, and two sets of sexual characters, one of which is patent and the other latent, according to the sex of the individual. This same condition Dr. Reid finds in Mendelian inheritance, the dominant characters being patent ones and the recessive latent, and he carries the idea a step further in maintaining that instead of sexual characters being Mendelian, these latter characters are sexual, the Mendelian phenomena depending on their relative potency or latency, rather than on the presence or absence of definite determining factors. "Unit segregation, gametic purity and independent inheritance of characters (in the Mendelian sense) are all myths that have been founded on experiment, but have not been tested by it or in any other way"!

In discussing mutations, to which class of variations he assigns those characters that show Mendelian phenomena, Dr. Reid maintains that "In hardly a single instance has the crossing of natural varieties revealed a latent ancestral character," that is to say, a recessive parental character! But in domestic races this revelation is frequent and therefore natural and artificial selection are essentially unlike. Man in dealing with domestic races uses mutations, but nature uses fluctuating variations. Mutations occur in nature, but "never yet has a mutation been recorded—neither in man, nor in lower animals, nor in plants—that gave its possessors an advantage in the struggle for existence so overpowering that it enabled them to supplant the ancestral type."

In what has been said the attempt has been made to state concisely Dr. Reid's position with regard to the principal problems of inheritance, and the abstract corresponds to about two fifths of the book. The remainder is occupied with discussions of the sociological and psychological applications of the author's conclusions, and concerning these, interesting though they are, space remains but for an illustrative statement and a quotation. The author holds that disease, alcohol and narcotics are the only important selective factors in the case of the human species, and the only progressive evolution that human races undergo is that which tends to resistance to these factors. Acquired immunity or total abstinence will not lead to the development of that resistance, and, far from being for the good of the race, would, if effective, expose it eventually to disaster from the very causes it endeavored to avoid. A newly introduced disease if fatal, is always more so than one to which the community has been for some time exposed and to which it has, by natural selection, gained some resistance.

"In considering any practical problem we must first of all determine what we propose to improve—whether germinal potentialities or characters which developed under the stimulus of nutrition, or of use, or of injury—and then consider in what way they may best be im-

proved—whether by selection or by altering the stimulus, and if the latter, how the stimulus may best be altered." Would that all sociological reformers might read and ponder these words.

J. P. McM.

REPORT OF THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE [1]

During the Graz meeting of the International Zoological Congress, the International Commission on Zoological Nomenclature held five executive sessions and one public meeting.

The following ten commissioners were present: Blanchard (president), Dautzenberg, Hoyle, Jentink, Jordan, von Maehrenthal, Monticelli, Schulze, Stiles (secretary) and Wright.

The following five commissioners were not in attendance: von Graff, Joubin, Osborn, Stejneger and Studer.

Resignations.—The following commissioners have presented resignations, and the commission recommends that their resignations be accepted: von Graff and Osborn.

Expiration of Term of Service.—The term of service expires at the close of this congress for the following five members of the class of 1910: Blanchard, Joubin, Stiles, Studer, Wright.

Nominations.—The following members of the congress are nominated to fill vacancies on the commission, caused by resignation or by expiration of term of service:

Class of 1913: Hubert Ludwig (Bonn), *vice* von Graff (Graz) resigned. J. A. Allen (New York), *vice* Osborn (New York) resigned.

Class of 1919: *vice* class of 1910 (term expired): R. Blanchard (Paris), C. W. Stiles (Washington), Louis Dollo (Brussels), Ernest Hartert (Tring), G. A. Boulenger (London).

By-laws.—The commission has adopted the following by-laws, based chiefly upon the methods of procedure adopted at former meetings:

[1] This report was read once in the open meeting of the commission and again in the last general session of the congress. It was adopted by the congress.—C. W. S.

BY-LAWS OF THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE

Article I. *Membership*

Sec. 1. This commission shall consist of fifteen members, elected by the International Zoological Congress.

Sec. 2. The commissioners shall serve in three classes of five commissioners each for nine years, so that one class of five commissioners shall retire at every international congress. The retiring commissioners may, however, be reelected to succeed themselves.

Sec. 3. In case of resignation or death of any commissioner, his place shall be filled for the unexpired term by the next international congress.

Article II. *Officers*

Sec. 1. The officers shall consist of a president and a secretary, elected by the commission from its members, to serve during their term as commissioners.

Sec. 2. The two officers shall form an executive committee, whose duty it shall be to perform such work as may from time to time be designated by the commission.

Article III. *Powers of the Commission*

Sec. 1. The commission shall have no legislative power but shall study the general subject of the theory and practise of zoological nomenclature, and shall report its recommendations to the triennial International Zoological Congress.

Sec. 2. The commission shall not report to any congress any proposition for amendment to the International Code, unless said proposition has been before the commission for at least one year prior to the meeting of said congress.

Sec. 3. The commission is authorized to express opinions on cases of nomenclature submitted to it.

Article IV. *Reports to the Congress*

Sec. 1. The commission shall make a report to each triennial International Zoological Congress. Said report shall consist of the following:

(*a*) Recommendations involving any alteration of the Regles Internationales de la Nomenclature Zoologique, but no such opinion is to be reported unless it has first received a majority (eight votes) of the commission and the unanimous vote of all commissioners present at the meeting.

(*b*) All opinions which have been rendered since the preceding congress.

(*c*) A list of all commissioners whose term of

service expires and of all vacancies caused by resignation or death.

Sec. 2. Said report (Art. IV., Sec. 1) shall be posted on a bulletin board as early as possible during the meeting of the congress and prior to the public meeting of the commission (Art. V., Sec. 1).

Article V. *Public Meeting*

Sec. 1. Prior to the request by the commission that its report be adopted and its opinions be ratified by the congress, the commission shall hold a public meeting, at which the opportunity to be heard on its report shall be granted to any member of the congress.

Article VI. *Majority Vote on Opinions*

Sec. 1. A majority vote (namely eight) of the entire and full commission is necessary for the adoption of any opinion. [This is a new proposition and is intended to preserve conservatism. For instance, suppose only thirteen members should vote; it is clear that seven would be a majority, but by the proposed Art. VI., Sec. 1, no "opinion" is adopted unless it has eight votes.]

Sec. 2. If, however, any opinion involves a reversal of any former opinion rendered by the commission, such opinion shall require the concurrence of at least twelve commissioners voting on same.

Article VII. *Publication of Opinions*

Sec. 1. After 90 days from date of mailing the opinions, as soon as a majority (eight) vote in favor of any opinion is returned to the secretary, said vote may be announced.

Article VIII. *Change of By-laws*

Sec. 1. The by-laws of this commission may be amended at any time by an affirmative vote of twelve members.

Financial Aid from the Smithsonian Institution.—Owing to the amount of clerical work connected with the studies conducted by the commission, it has been found very difficult in the past for the commission to render its decisions as promptly as desirable. This difficulty has now been overcome by the generous grant of the sum of $2,700 by the Smithsonian Institution; said sum is available at the discretion of the commission at any time during the three years following the grant.

In addition, the Smithsonian Institution has placed at the disposal of the commission the sum of $500 to be used in publishing the "opinions" rendered by the commission in its function as a court of appeal. An arrangement has been made between the secretary of the Smithsonian Institution and the secretary of the commission, whereby the "opinions" will be published by the institution and forwarded to 1,100 libraries, to the members of the International Zoological Congress, and to a limited list of specialists.

Opinions Rendered.—Since October, 1909, the commission has rendered twenty "opinions" (Nos. 6–25), which are now in press and which will soon be sent to all members of the congress. A number of cases are still before the commission for study and will be passed upon in the near future. [At this point the report contained the summaries of all "opinions" rendered since the Boston meeting.]

Official List of most Frequently used Zoological Names.—There is a desire on the part of some zoologists that certain very commonly used zoological names should be excepted from the application of the *law of priority*, and a proposition to this effect has been presented to the commission from the British Association for the Advancement of Science and the Eastern Branch of the American Society of Zoologists. That this desire is so wide-spread and so deeply rooted as is assumed by some of our colleagues has not been confirmed by inquiries made by several members of the commission. Further, an effort made by the secretary to collect from zoologists the most commonly used and most important generic names has as yet met with such poor success that the conclusion does not seem entirely unjustified that some of our colleagues who may be in favor of such a list are not as yet sufficiently enthusiastic over the proposition to induce them to demonstrate their desire by placing in the hands of the commission the data upon which such a list must of necessity be based. Further, there are many colleagues who are known to us to be directly and enthusiastically opposed to such a list.

After careful consideration of the subject and of the many difficulties involved, the com-

mission has decided to propose to the congress the trial of a proposition which it is hoped will meet with the approval of both sides of the controversy, namely:

1. The commission invites all zoologists to send to the secretary of the commission, prior to November 1, 1910, a list of 100 zoological generic names which they consider should be studied in connection with the preparation of an "official list." Each name should be accompanied either by the name of the author of the generic name, or by an indication of the group to which it belongs.

2. All systematists are invited to send a separate list of the 50 to 100 generic names in their specialty which they look upon as the most important and most generally used. Each name should be accompanied by the full and complete original bibliographic reference, by the name of the type species, determined according to article 30 of the international rules, and by the name of the order and family to which the genus belongs.

3. All zoologists and paleontologists who give courses in general zoology are invited to supply the secretary with a list of the text-books used in said courses so that said books may be indexed for generic names.

4. The commission will alphabetize all the generic names sent in and will endeavor, according to circumstances, to determine which are the 100 to 500 most commonly quoted genera.

5. The genera selected will be submitted to specialists in the groups in question who will be requested to submit opinions on the nomenclatorial status of said names.

6. Upon return of the lists from the specialists, the commission will endeavor to test the names, according to the international rules, and if feasible will publish a list of the genera in question with their most commonly used names and their correct names.

7. If the undertaking is successful, the zoologists of the world will be invited to give to the commission the benefit of their criticisms not later than July 1, 1912, so that the commission can restudy the names and submit to the next congress—

8. *An official list of generic names*, with their genotypes; and with the

9. Proposition that the congress adopt said list and a

10. Resolution to the effect that no zoologist shall upon *nomenclatorial* grounds change any name in said list unless he first submits to the commission his reasons for making the change and unless the commission considers the reasons valid.

The commission believes that this proposition is feasible, but for the present views it in the light of an experiment, dependent to no small extent upon the question whether a proper amount of cooperation is forthcoming. In this connection the commission takes the liberty of inviting attention to the fact that the great advances in nomenclature have been made by colleagues who have shown a conviction in their views sufficient to induce them to devote some time to the subject.

Amendments to the "Regles internationales de la nomenclature zoologique."—In its executive sessions the commission has considered thirty propositions which have been submitted as amendments to the present international rules. Of these propositions, the commission unanimously recommends to the congress the adoption of the following:

Art. 4: For the word *root*, substitute the word *stem*.

Art. 27 (*b*): For the word *larva*, substitute the words *any stage in the life history*.

Art. 35: Insert as a third paragraph the following:

"Specific names of the same origin and meaning shall be considered homonyms if they are distinguished from each other only by the following differences:

(*a*) The use of *æ*, *œ* and *e*, as *cœruleus, cæruleus, ceruleus; ei, i* and *y*, as *chiropus, cheiropus; c* and *k*, as *microdon, mikrodon*.

(*b*) The aspiration or non-aspiration of a consonant, as *oxyryncus, oxyrhynchus*.

(*c*) The presence or absence of a *c* before *t*, as *autumnalis, auctumnalis*.

(*d*) By a single or double consonant: *litoralis, littoralis*.

(e) By the endings *ensis* and *iensis* to a geographical name, as *timorensis, timoriensis*.

Art. 36: Omit from the examples—*Macrodon, Microdon; cæruleus, cœruleus, ceruleus; silvestris, sylvestris, silvaticus, sylvaticus; littoralis, litoralis; autumnalis, auctumnalis; dama, damma*.

Appendix F: In the English and German texts, substitute the words *transliteration* and *transliterated* for *transcription* and *transcribed*.

Appendix G: In all three texts, substitute *paragraph* for *rules*, and omit from the heading in French text the words *Regles de la*.

Italian Translation.—The commission has voted to issue an official Italian edition of the international rules.

CH. WARDELL STILES,
Secretary of Commission

SPECIAL ARTICLES

PRELIMINARY NOTE ON THE PERMEABILITY TO SALTS OF THE GILL MEMBRANES OF A FISH

IT is known that when marine fishes are placed in fresh water there is a gain in weight supposed to be due to the absorption of water. Sumner (1905) has obtained evidence tending to show that the water enters the body chiefly through the gill membranes. Experiments by one of the authors of this note tend to confirm this. Sumner (1905) has also compared the chlorine content of such fishes (analyzing the ash obtained by fusing the entire fish) with the chlorine content of the normal fish and has reported a loss in chlorine, indicating that while there was a movement of the fresh water into the body of the fish through the gills there was at the same time a passage of salts outward—in other words the gill membranes seemed to be permeable to salts.

In a series of experiments carried out at the Biological Laboratory of the U. S. Bureau of Fisheries at Woods Hole, Mass., the authors have obtained further evidence along this line, experiments of the following nature being most significant. A quantity of blood was taken from the caudal artery of a large specimen of *Mustelis canis*. The specimen was then placed in a sea-water tank (the caudal part of the body not being immersed and loss of blood being prevented) and a stream of fresh water was then turned into the tank, the salt water being turned off so that it was replaced by the fresh water in about fifteen minutes. The specimen was kept in this fresh water for thirty minutes, when a second sample of blood was obtained from the caudal artery. The specimen was then returned to the fresh water for forty-five more minutes and a third sample was then taken. Analysis of the blood was begun in each case immediately after the sample was obtained. Following are the results:

Sample 1. Normal blood (*i.e.*, from fish just taken from sea-water).

Sample 2. Blood from same specimen after immersion in salt-fresh to fresh water for 45 minutes.

Sample 3. Blood from same specimen after immersion in fresh water for 45 minutes more.

GRAMS PER 1,000 GRAMS OF BLOOD

	Water	Organic Matter	Chlorides
Sample 1	= 868	118	6.041
Sample 2	= 881	110	4.132
Sample 3	= 885	104	3.590

The greater amount of water in the second sample shows a dilution of the blood. The blood is further diluted in the third sample. There is no question then about the absorption of water. Since the blood is diluted we should expect to find less organic matter. This decrease is shown in the second column and was obtained by subtracting the weight of the ashed sample from the weight of the dried sample and reducing to grams per 1,000. The actual amount of chlorides was obtained by the Volhard method and then reduced to grams per 1,000. The results are shown in the third column. Since we should expect a diminution of the salts, provided water is added to the blood, the diminution shown above may be partially explained in this way. But it can be seen at a glance that the chlorine reduction is out of proportion to the decrease in organic matter. If the organic matter is reduced from 118 to 110 by simple

dilution, to get the same degree of dilution the chlorides would be reduced from 6.041 to 5.631 grams per 1,000. As a matter of fact, the actual amount of chlorides found was 4.132 grams per 1,000 or 27 per cent. less than if it were a case of simple dilution. Again applying the same method to the third sample, if it were a case of dilution alone we should expect 5.324 grams per 1,000 grams blood, whereas analysis shows but 3.590 grams, or about 33 per cent. less chlorides than if it were a case of simple dilution. The salts would not disappear in the tissues, for if anything the tissues would be surrendering their salts to the blood stream in an endeavor to keep up the osmotic pressure of the blood. We are therefore forced to conclude that the chlorides passed out through the gills—in other words, the gills are permeable to salts.

G. G. Scott,
College of the City of New York
G. F. White,
Richmond College (Virginia)

PÆDOGENESIS IN TANYTARSUS

As the phenomenon of pædogenesis in the Chironomidæ is rarely observed, it may be of interest to zoologists to know that we have a species of rather wide distribution in which this mode of reproduction seems to be of common occurrence. In the summer of 1903 at Ithaca, New York, while studying the Chironomidæ, I several times came upon the larva of *Tanytarsus dissimilis,* which, when placed in a tumbler of tap water gave rise to a number of individuals. The same year the late Dr. Fletcher, dominion entomologist, sent me some adults of the same species for identification which he said had developed pædogenetically.

This summer at Orono, Maine, I found a number of them in a jar in which some *Dixa* larvæ were kept. They appear to have been the progeny of a larva introduced by chance. One individual of this generation after careful examination was transferred to another jar containing distilled water, a bit of sterilized vegetable débris serving as food. These precautions were taken in order to prevent eggs or small larvæ being carried over. After about two weeks a number of minute trails were observed, each containing a young *Tanytarsus* larva, a new generation appearing simultaneously also in the first jar. Though I have reared many species of *Chironomidæ,* I have never observed this method of reproduction in any other species. In this connection it is interesting to note that Professor Zavrel quite recently ('07) published an account of pædogenesis in *Tanytarsus* occurring in Bohemia. It is quite possible that the species with which Grimm worked also belonged to the same genus.

The larvæ are usually to be found in the mud and sediment in pools where *Anopheles* might live. Jars containing cultures of Protozoa are sometimes seen with a number of the characteristic trails or tubes of *Tanytarsus* larvæ on the sides of the glass. The tube is composed of fragments of decaying plant tissue and is usually several times longer than the larva which inhabits it. If the tube be disturbed the insect wriggles out and swims away by violent contortions of its body. When full grown it is about 3.5 mm. long, of a pale amber color and is readily distinguished from other related forms by its relatively long, non-retractile antennæ and the form of its mouth parts. The pupa is characterized by the arrangement of the setæ on the dorsum of the abdominal segments, most readily seen in a cast skin. The adult is about 1.5 mm. in length, yellowish-green in color with three brown thoracic stripes, and though common enough, owing to its small size is but rarely seen. More extended descriptions of the three stages may be found in Bulletin 86, New York State Museum (1905).

I have long delayed publishing my notes on this insect thinking that I might sooner or later chance upon larvæ in which the young were developing, but as lack of time prevents my making a systematic search I now write this in the hope that it may put someone else upon the track of this interesting species.

O. A. Johannsen
Maine Agricultural
Experiment Station